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ANNEX

ANNEX

A
JOURNAL
OF
NATURAL PHILOSOPHY,
CHEMISTRY,
AND
THE ARTS.

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VOL. III.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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1802.

PREFACE.

THE third Volume of this Miscellany being now completed, I have again the opportunity of making my acknowledgements to its friends and supporters, and presenting the respectable list of its Authors.

The authors of original Papers are John Bostock, M. D. Mr. Rob. Jameson; the Rev. James Wilson, A.M.; John Gough, Esq.; Mr. Ez. Walker; Rev. Joseph Priestley, L.L.D. F.R.S.; Rev. W. Pearson, P. R. I.; Dr. Thomas Young, F. R. S. and Professor at the Royal Institution; Mr. Peter Keir; Mr. John Dalton; Mr. Wm. Wilson; R. Chenevix, Esq. F. R. S.; Mr. John Cuthbertſon; Mr. John Murray; J. B.; W. N.; and Profeſſor Aldini.—Of foreign works; Hapel Lachenaie; Brongniart; J. B. Berard; Guyton; Gay Lussac; Ekeberg; B. Prevost.—And of English Memoirs abridged or extracted, Dr. Young; Mr. R. Phillips; Mr. J. Besant; Edw. Howard, Esq.; F. R. S.; Sir H. C. Englefield, Bart. F. R. S.; Mr. J. Dalton; Mr. H. Davy, Profeſſor at the Royal Institution; T. Wedgwood, Esq.; R. Chenevix, Esq. F. R. S. Mr. Wm. Henry.

Of the Engravings the Subjects are, 1. A powerful Blowpipe by Alcohol. 2. Mr. Wilson's two new Barometers. 3. An improved Water Wheel, by Mr. Besant. 4. Mr. R. Phillips's Tubes for driving Copper Bolts into Ships. 5. The Hydrometer of Atkins, for exhibiting Specific Gravities and

Strengths of Spirits without Computation. 6. Mr. Read's very simple and cheap Pneumatic Apparatus. 7. The new moveable Crane erected by Mr. Keir at Ramsgate. 8. Haas's Blow-pipe Apparatus. 9. Figures by Mr. Jameson, to illustrate the Formation of Basalt. 10. Elements of the Galvanic Pile, by Dr. Bostock. 11. Distinguiſhers of Galvanism and Electricity. 12. Gay Lussac's Apparatus for measuring the Expansions of the Gases. 13. The Indian Hand Mill. 14. A Secret Lock, which can be opened only by the Proprietor. 15. Improvement of Woulfe's Apparatus, by Mr. Murray. 16. Dr. Young's Apparatus for exhibiting the colours of thin plates. 17. Figures by Prevost, for explaining the Phenomena of Dew.

Soho Square, Dec. 26, 1802.

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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

SEPTEMBER, 1802.

ARTICLE I.

Deſcription of a cheap and ſimple Apparatus or Blow-pipe, in which the Flame of Oil or Tallow is impelled by the Vapour of Alcohol. From a Correſpondent.

To Mr. NICHOLSON.

SIR, *London, July* 9, 1802.

Inconveniences of the blow-pipe, and the ſubſtitutes commonly uſed.

USEFUL as the *blow-pipe* is for the profeſſed mineralogiſt and chemiſt, as well as for the amateur of the ſcience, it is, however, liable to the inconvenience of requiring one hand of the operator to be employed; beſides, that perſons of weak lungs, or not well accuſtomed to its uſe, find it laborious, even when they attempt to manage it according to the directions given by Bergman and Engſtrom; more eſpecially, when a continued ſtream of air is required for ſome time. For this reaſon, ſome have recourſe to a *bladder* filled with air, and force it out by preſſing the bladder between the arm and their body; others uſe a ſmall pair of *double bellows*, to be fixed upon the table: others again employ a receiver, or rather *gazometer*, in the form of Mr. Watt's hydraulic bellows. But all theſe means are not always ready at hand, or not eaſily portable to accompany the travelling mineralogiſt, and in every caſe they require ſome preparation. And even the ingenious contrivance of Mr. Haas, though it permits the free uſe of the operator's hands, is expenſive, and ſtill requires the blowing through the mouth.

Very uſeful lamp exhibited by Profeſſor Pictet.

When Profeſſor Pictet of Geneva viſited London laſt year, he brought along with him and exhibited to his friends an apparatus, which at the ſame time that it is ſimple and very portable, is the moſt advantageous ſubſtitute for the blow-pipe, and removes all its inconveniences. An imitation of this apparatus was exhibited in the lecture-room of the Royal Inſtitution, Albemarle-ſtreet.

Deſcription of two lamps for blowing the flame of oil by alcohol.

The drawing of this inſtrument which I ſubjoin, was copied from that communicated by a friend. A, plate 1, is a tin-box filled with common oil, ſerving to feed the flame at F, thence to be directed on the ſubſtance under examination, and at the other flame, to heat the ſpirit of wine contained in the copper or tin lamp B. The boiling ſpirit being converted into ſteam by the heat, ruſhes through the bended tube C upon the flame of the wick at F. Laſtly, D is a ſlider with loops and ſcrews to raiſe or lower the ſpirit lamp. The whole being very ſimple and clear, any farther explanation would be needleſs.

Long ago uſed by Nollet.

I do not know whether Profeſſor Pictet pretends to be the inventor of this apparatus; but certain it is, that Abbe Nollet more than fifty years ago employed ſuch a contrivance, as may be ſeen in his *l'Art de faire Experiences*. Another application of the ſpirit lamp for this purpoſe I have read in the *Annales de Chimie*, but do not recollect in which volume.

There is no doubt but the communication of the lamp above deſcribed will be acceptable to many of the readers of your excellent Journal; whence, if you agree with me in this opinion, be ſo kind as to inſert it.

I am, Sir,

Your humble ſervant,

N. N.

ANNOTATION. W. N.

Powerful effect of a ſmall eolipile.

The application of the elaſtic and combuſtible vapour of alcohol to this uſeful purpoſe is ſo obvious, that it can ſcarcely be appropriated as an invention. The glaſs-blowers have long ſold a little implement, conſiſting of an egg-ſhaped ball, having a ſtem a ſtem or handle, and a nozzle or tube. It is an eolipile, and when half filled with water or ſpirit, and heated over a candle, a ſtrong current either of vapour or of the denſe fluid will iſſue out accordingly as it is held. With the vapour of ſpirit thus blown through the large flame of a tallow lamp, I fuſed a halfpenny, and ſoftened the ſtopper of a bottle ſo as to run a wire through it; though I muſt confeſs not without apprehenſion of the thin brittle veſſel burſting, and covering the ſurrounding bodies with flame. The lamp B, which is the eſſential part, may be made cheap; and indeed the whole would coſt but a trifle. I took a ſmall lamp of that figure, 2½ inches wide and 1½ inch high (of the ſort called *agitable*, from the cap for the wick being made to ſcrew in and defend the oil), and in the place of the wick-pipe, I ſoldered a piece of a common blow-pipe. The lamp coſt eighteen pence, and the blow-pipe ſix-pence, and the combination anſwers perfectly well.

An halfpenny melted, and a glaſs ſtopper perforated.

The lamp apparatus is very cheap.

II.

Outline of the Hiſtory of Galvaniſm: with a Theory of the Action of the Galvanic Apparatus. By JOHN BOSTOCK, *M. D. From the Author.*

(*Concluded from Page* 304.)

COL. HALDANE found that the effects of the apparatus were ſuſpended when it was immerſed under water, and that this was alſo the caſe when it was placed under the vacuum of an air-pump. He conceived that the effects of the pile were not increaſed by its being connected with the conductor of an electrical machine, and he thought that the charging of the Leyden phial was retarded when the apparatus was placed between its outſide and inſide coatings. Upon the whole he concludes that its effects cannot be referred to electricity. Upon theſe experiments Mr. Nicholſon remarks, that in the caſe where the pile was connected with the conductor, the electric ſtream of the conductor might have been in a contrary direction to the electric ſtream of the pile. In the experiment where the pile was connected with the Leyden phial, it is certain that the phial could not be charged higher than the pile

Col. Haldane immerſed the pile in water. He diſcovered that its action is ſuſpended in vacuo.

Thinks it not electric.

Obſervations to the contrary.

itſelf, and from the effects of galvaniſm in general, it may be conjectured that it reſembles a large ſurface of an electric charged to a low intenſity.

Cruickſhank's ſimple apparatus for keeping the gaſes diſtinct.

His general concluſions. 1. Silver end of the pile gives chiefly hidrogen. 2. Reduction of metallic ſolution at this end. 3. Zinc end gives chiefly oxigen. 4. Or produces oxidation.

Mr. Cruickſhank, in a ſecond communication, gives an account of ſome experiments which he performed with an interrupted wire of gold. By means of a bent glaſs tube he contrived to keep the gaſes obtained from each wire perfectly diſtinct. That from the ſilver end of the pile he found to be chiefly hidrogen, and that from the zinc end nearly pure oxigen. Both the gold wire and one which he uſed of platina were evidently tarniſhed on the zinc end, after being expoſed for ſome time to the influence. He draws the following general concluſions from his experiments. *1ſt*, From the ſilver end, if it be immerſed in water, whatever be the nature of the wire, there is diſengaged a quantity of hidrogen gas mixed with a ſmall quantity of oxigen gas and ammonia. *2d*, Where a metallic ſolution is uſed inſtead of water, the metal is reduced and is depoſited at the end of the wire. *3d*, When the wire at the zinc end conſiſts of gold or platina, oxigen gas is diſengaged mixed with a little azote and nitrous acid; the quantity of gas is only about one-third of what is diſengaged at the ſilver end, ſo that the gaſes exiſt in the proportion requiſite to produce water. *4th*, When the zinc wire is formed of an imperfect metal, it is partly oxidated, partly diſſolved, and a little oxigen only is diſengaged. Fluids which contain no oxigen Mr. Cruickſhank thinks are incapable of tranſmitting the influence, whilſt all thoſe which contain it appear capable of conducting the influence. Mr. Cruickſhank then proceeds to give an account of a new method of placing the metals horizontally in a kind of trough, which in ſome reſpects appears to be more convenient than the form of the apparatus originally invented by Volta.

His Invention of a new galvanic arrangement by a trough.

Mr. Davy obtains the oxigen from one portion of water, and the hidrogen from another unconnected portion.

Mr. Davy propoſed this curious ſubject of enquiry, whether the oxigen and hidrogen gaſes evolved by the interrupted wire when immerſed in water, can be procured from two portions of water kept diſtinct from each other? For this purpoſe he cauſed the wires to terminate in two glaſſes of water, and by means of his fingers eſtabliſhed a communication between the two glaſſes. The gaſes he found were produced as in the former caſe. When the water uſed was very carefully boiled, he found that the gaſes evolved were nearly pure, and in the proportions requiſite to form water. Mr. Davy then wiſhed to aſcertain whether it was eſſential for the wires to be in contact with the metallic plates of the pile; the ends of the pile were therefore made to communicate with two glaſſes of water by pieces of muſcular fibre, and the glaſſes were connected by a ſilver wire. The effects of the pile were now reverſed. Hidrogen was diſengaged by the wire at the zinc end, while the wire at the ſilver end became oxidated.

Reverſe effect on a ſingle intermediate wire.

Col. Haldane, in a ſecond memoir, details a ſeries of experiments made to determine the power of different combinations of metals. He found the pile acted more powerfully when immerſed in a given quantity of oxigen gas, than when confined in the ſame bulk of atmoſpherical air; its action was entirely ſuſpended in azote or in a vacuum. On this account he conjectures that the effects of galvaniſm depend upon a chemical operation, and that oxigen is attracted by the apparatus from the atmoſphere.

Col. Haldane ſhews that the pile acts more ſtrongly in oxigen, and not at all in azote or in vacuo.

Mr. Davy diſcovered that the influence of the pile was capable of being tranſmitted through charcoal, when an apparatus was conſtructed of this ſubſtance and ſilver. It decompoſed water in the uſual manner, the ſilver end giving out hidrogen with a little carbon in ſolution, while the zinc end evolved but little gas of any kind; this was probably owing to the abſorption of the carbonic acid which would be formed at this end of the wire. He found that when the water between the pairs of metallic plates was perfectly pure, *i. e.* when it contained neither acid, ſalt, nor gas of any kind, the pile ceaſed to act. He conjectures that the energy of the pile is nearly in proportion to the rapidity with which the zinc becomes oxidated; and conſequently the effects were found to be the moſt powerful when nitric acid was interpoſed between the metals. Mr. Davy appears in this inſtance to have made the firſt ſtep towards the true theory of the action of Volta's pile: as might be inferred from this notion, he found that concentrated ſulphuric acid interpoſed between the metals, acted leſs powerfully than diluted ſulphuric acid, becauſe this laſt has more power in oxidating zinc than when in its pure ſtate. He alſo diſcovered that the pile can act in vacuo, provided a little acid be interpoſed between the plates. A pile compoſed of zinc and charcoal was found to poſſeſs great energy.

Davy's application of charcoal as the conductor,

His diſcoveries that pure water is ineffectual in the pile, and that its effects are proportioned to the oxidation.

He applies the acids,

and cauſes the pile to act in vacuo.

All theſe valuable diſcoveries and experiments are taken from original papers communicated to the 4th vol. of Nicholſon's Journal.

Anonym. that women are more ſuſceptible of galvaniſm than men.

In the 5th vol. of this work we meet with ſome curious obſervations detailed by an anonymous correſpondent at Edinburgh. He found that water which had been for ſome time ſubjected to the influence of the pile, continued for ſeveral days to emit gas, and to depoſit a mucous matter. He has obſerved that when both men and women form the galvanic circuit, the women appear to receive a larger ſhare of the ſhock than the men. Dr. Rutherford of Edinburgh conceives that vegetables are non-conductors of galvaniſm, and therefore in this particular that it differs from electricity.

Mr. Davy's pile with one metal only.

Mr. Davy afterwards diſcovered that a pile may be conſtructed with only one metal, provided proper fluids be applied to its ſurfaces. He employed the following ſeries: metal, nitrous acid, water, ſulphuret of potaſh, and then metal. This curious diſcovery, as Mr. Nicholſon judiciouſly obſerves, completely overthrows the hypotheſis of Volta.

Cruickſhank charges a jar with galvaniſm.

Mr. Nicholſon about this time was informed by Mr. Cruickſhank, that he had at length ſucceeded, by uſing proper precautions, in charging the Leyden phial by means of the galvanic pile.

Hitherto nearly all the wonderful diſcoveries that had been made by means of Volta's apparatus were confined to England, and were principally due to Mr. Nicholſon, Mr. Cruickſhank, Mr. William Henry, Mr. Davy, and Col. Haldane. The ſubject began now to attract the attention of foreigners, and we have an account of ſome curious experiments performed by Profeſſor Tromſdorff.

Tromſdorff burns gold leaf and other metals.

Remarkable diſcovery of Fourcroy, that the ſhock is greateſt when the repetition of plates is moſt numerous; the combuſtion greateſt the larger the ſurface.

He conſtructed a pile of zinc and copper, and fixed a piece of gold leaf to the zinc end of the wire; he found that it was eaſily conſumed, and that the ſame thing happened to other metallic leaves. This experiment was repeated by Fourcroy, and he adds this remarkable circumſtance, that ſix large zinc and ſilver plates gave only a ſlight ſhock, but conſumed the wire rapidly; whereas the ſame ſurface of metal arranged in the form of a greater number of ſmaller plates, afforded a more conſiderable ſhock, but would not burn the metal.

Cruickſhank obſerves two dif-

About this time Mr. Cruickſhank obſerved, that when the wire of his trough apparatus terminated at the ſurface of the water, ferent kinds of ſpark.

water, the ſilver end emitted from the water a bruſh of fire, at the ſame time that the zinc end produced only a denſe ſpark. This circumſtance induced him to ſuppoſe that the ſilver was in the plus and the zinc in the minus ſtate of electricity, which indeed agrees with Bennet's original experiments, though it diſfers from the account given by Mr. Nicholſon in his firſt paper, and alſo from the experiments of Dr. Wollaſton, Dr. Van Marum, and others, as we ſhall ſee below.

Dr. Wollaſton.

Oxidation diſengages electricity,

Dr. Wollaſton read a judicious paper to the Royal Society, which appeared in the Tranſactions for the year 1801. He remarks, as Mr. Davy had previouſly done, that the energy of the pile ſeems proportionate to the diſpoſition which one of the plates has to be oxidated or acted upon by the interpoſed fluid. If a piece of ſilver and a piece of zinc be immerſed in diluted ſulphuric acid, the zinc immediately begins to decompoſe the water and to evolve hidrogen; the ſilver is not acted upon, but if we bring them into contact, the ſilver alſo begins to produce hidrogen. He therefore concludes, that in the ſolution of a metal by an acid, electricity is always diſengaged. In order that the electrical machine ſhould act ſtrongly, it is neceſſary that the amalgam upon the cuſhion ſhould be compoſed of a metal which is eaſily oxidated; the Doctor alſo found that an electrical machine plunged into carbonic acid gas is incapable of being excited. He concludes by the opinion which was ſuggeſted by Mr. Nicholſon, that electricity and galvaniſm are the ſame principle, but that in the latter it is generally met with in large quantity, but in a ſtate of low intenſity. Dr. Wollaſton coated two ſilver wires, leaving the ends only of them expoſed; theſe wires were connected with the two conductors of an electrical machine, and the ſpark was taken from one to the other through a ſolution of copper: he found that the end of the wire connected with the negative conductor was covered with reduced copper; no change had taken place in the other end. The negative conductor appears therefore, from theſe experiments, to correſpond with the ſilver end of the pile.

from the amalgam of the el. machine;

which will not act in carbonic acid.

He firſt produced the galvanic effect on water by common electricity.

Mr. Davy's pile with one ſingle metal,

Mr. Davy's very curious diſcovery, that a galvanic pile might be conſtructed by one metal alone, provided its different ſurfaces were expoſed to the action of different fluids, has been already announced: In addition to theſe facts the ſame intelligent experimenter informs us, in the firſt vol. of Nicholſon's Journal, N. S. that he has conſtructed a galvanic apparatus

which

and of charcoal with no metal.

which contains no metal, and which conſiſts ſolely of pieces of charcoal, which have their different ſides expoſed to the action of different fluids.

Volta's account of his own theory.

In the ſame volume there is an account of a paper written by Volta himſelf; but from the opinions which are advanced in it, we may conclude that he was unacquainted with many of the moſt intereſting circumſtances which had been obſerved in this country, reſpecting the operations of the apparatus originally diſcovered by himſelf.

Dr. Van Marum charges a large battery by galvaniſm.

In the 40th volume of the Annales de Chimie, is a letter from Dr. Van Marum to Sig. Volta, containing an account of experiments performed in order to compare the effects of the galvanic pile with thoſe of the great Teylerian machine at Haarlem. Both ſingle jars and batteries were charged by means of the galvanic pile, and in all caſes they were charged to the ſame degree of intenſity with that which the pile itſelf indicated to an electrometer. When the zinc was at the top of the pile, and communicated with the inſide of the jar, the electricity of the inſide was found to be poſitive, and when the pile was reverſed, it was negative. It was found that the ſhocks given by the battery charged from the electrical machine, were not perceptibly different from thoſe given by the battery when charged to an equal intenſity by the pile. By conſtructing a pile of large plates of zinc and copper, he was enabled to fuſe thick iron wires; he even ſucceeded in fuſing a wire of platina. He found that piles which conſiſt of the ſame number of plates, but of different diameters, give equal intenſities and equal ſhocks; yet thoſe with the larger plates he found to be conſiderably more powerful in fuſing metals.

Dr. Prieſtley's experiments.

In the firſt volume of Nicholſon's Journal, N. S. is a letter from Dr. Prieſtley on the pile of Volta. It contains a number of intereſting experiments performed by this venerable philoſopher, principally with a view of eſtabliſhing his favourite hypotheſis of phlogiſton, to which he ſtill adheres. Some of the facts which he notices had been previouſly obſerved in England, though from his remote ſituation he had not the opportunity of becoming acquainted with them. He interpoſed four glaſſes of water between the two ends of the pile; the glaſſes were connected by means of ſilver wires, and the uſual operations went on at the legs of the wires; but it was found that when one of the portions of water had its ſurface covered with oil,

Galvaniſm ſaid not to act on water under oil;

oil, the action was ſuſpended in the whole of the glaſſes. This experiment has not, however, been found to anſwer in England.

This not confirmed.

Theory of the Action of the Galvanic Apparatus.

THE phenomena of the galvanic pile, which at firſt view appear almoſt incapable of being arranged into any ſyſtematic form, may, I think, be all eaſily explained by admitting the truth of the three following *poſtulates*. *1ſt*, That the electric fluid is always liberated or generated when a metal or any oxidable ſubſtance is united to oxigen; *2dly*, That the electric fluid has a ſtrong attraction for hidrogen; and, *3dly*, That when the electric fluid, in paſſing along a chain of conductors, leaves an oxidable ſubſtance to be conveyed through water, it unites itſelf to hidrogen, from which it is again diſengaged when it returns to the oxidable conductor.

Elementary poſitions of galvaniſm.

1. Electricity is given out during oxidation. 2. Electricity ſtrongly attracts hidrogen. 3. Electricity paſſes (inviſibly) through water, in combination with hidrogen.

The firſt of theſe propoſitions may be conſidered as almoſt proved by the experiments of Fabroni, Davy, and Wollaſton; the ſecond and third have not been directly eſtabliſhed by experiment, but will not appear improbable when it is ſeen in what a ſimple and eaſy manner they account for all the phenomena hitherto obſerved. In endeavouring to explain the action of the pile, two diſtinct ſubjects of inquiry preſent themſelves: *1ſt*, What are the operations carried on at the ends of the wires in the interrupted circuit, as diſcovered by Mr. Nicholſon? and, *2dly*, What is the operation carried on in the body of the pile itſelf?

Nature of the proofs.

As the current of the electric fluid appears to paſs from the zinc or plus end of the apparatus to the ſilver end, we muſt firſt endeavour to aſcertain the action which takes place at the zinc end of the wire. This in fact appears to be the diſengagement of oxigen in a concentrated ſtate, by which the wire itſelf when oxidable, is corroded, but which, when the wire is formed of a perfect metal, is diſengaged in the form of oxigen gas. This oxigen appears to be derived from the decompoſition of the water in which the wire terminates, in conſequence of the attraction which the electrical fluid poſſeſſes for hidrogen, and its incapacity of paſſing through water without being united to this ſubſtance, according to the ſecond and third poſtulates.

Theory of what happens in Nicholſon's tube. El. paſſes from the z. end and decompoſes the water: the oxigen either appears or oxides the metal: —

The elect. and hidrogen paſs inviſibly to the other point, where the firſt is abſorbed and the latter becomes gas; or if the fluid contain oxide, it reduces the metal and again forms water.

The electric fluid thus united to hidrogen, is carried to the other point of the wire, where, upon entering the oxidable conductor, it is diſengaged in the form of hidrogenous gas, if water be the medium of communication; if a ſolution of a metallic oxide be employed, it unites with the oxide, and reduces it. The decompoſition of water is therefore effected at the zinc point alone, though the different gaſes which compoſe it are diſengaged at each of the points; and this proceſs will continue even when the points terminate in two different portions of water, as was diſcovered by Mr. Davy, provided that the glaſſes are united by a conductor which is not oxidable.

Theory of the apparatus or pile.

The oxidation of the zinc developes elect. which decompoſes the water, and is attracted by the hidrogen; and therefore paſſes to the ſilver *through the fluid*, and not the reverſe. Its paſſage to the next oxidating ſurface adds to the quantity of el. and increaſes the oxidation; and this effect will increaſe with the number of pairs.

This may be conſidered as a general explanation of the firſt ſubject of inquiry; we muſt next proceed to inveſtigate the nature of the operation carried on in the body of the apparatus itſelf. In the conſtruction of the pile there are two points which are eſſential to its action; 1*ſt*, That the electric fluid be diſengaged; and, 2*dly*, That it be confined and carried forward in one direction, ſo as to be concentrated in the end of the apparatus. The firſt object is evidently attained by the oxidation of the zinc or other oxidable body employed. If both ſides of the zinc were oxidated, the electric fluid would indeed be liberated, but it would be immediately diſperſed, and its effects could not be obſerved. As ſoon, however, as the electric matter is evolved, it is immediately attracted by the hidrogen, which is at the ſame time neceſſarily generated in the fluid which oxidates the metal, and it is by this means conveyed acroſs the water to the ſilver plate, when two metals are uſed; or, in other caſes, ſimply to the oppoſite ſurface of the oxidating ſubſtance. The electric fluid then enters the ſilver plate, and inſtantly paſſing on to the contiguous zinc plate, arrives at a ſecond oxidating ſurface. The ſame ſeries of events which have been deſcribed is here repeated, except that the electric fluid being in ſome degree accumulated in the metallic plate, is diſengaged by the ſecond oxidating ſurface in larger quantity, and in a more concentrated ſtate, than before. By purſuing the ſame train of operations, it is eaſy to ſee how the electric matter will continue to be accumulated in each ſucceſſive pair of plates, until, by ſufficient repetition, it may be made to exiſt in the zinc end of the pile in any aſſigned degree of force.

Having

Remarks a
inferences
this theor

Having thus exhibited a general view of the hypotheſis which has ſuggeſted itſelf to my mind, as affording a ſimple explanation of the phenomena, without offering violence to any of the generally received opinions reſpecting chemiſtry or electricity, I ſhall proceed to illuſtrate its truth, by applying it to the explanation of ſome of the moſt remarkable properties of the pile. It is ſufficiently obvious that upon theſe principles the action of the pile will continue as long as the ſurface of the zinc continues to be oxidated, and that, the ſize of the pile remaining the ſame, its energy will be proportionate to the rapidity of this operation. The real difference between the two ends of the apparatus will appear to be not ſo much a difference in the abſolute quantity of electrical matter contained in them, as a difference in the direction of its current; becauſe exactly the ſame quantity of fluid which paſſes out of the zinc end muſt be received at the ſilver end, when a communication is eſtabliſhed between them. It may be farther remarked, that when the pile is conſtructed as directed by Volta, beginning with zinc, then ſilver, card and zinc, &c. though the zinc forms the baſis of the pile, yet as no change takes place in the electric fluid during its paſſage from the ſilver to the zinc, this might with equal propriety have been called the ſilver end: in order, however, to avoid confuſion, the uſual language has been adopted in this paper; the terms *diſcharging* and *receiving* ends would be more appropriate.

Conſiderati
on the appa
tuſes of Da
and Cruick
ſhank,

It has excited a good deal of ſurprize, that, in the experiments of Mr. Davy and Mr. Cruickſhank, apparatuſes, conſtructed of various ſhapes and of ſuch very different materials, ſhould be capable of producing the ſame operations. We may in a few words enumerate the circumſtances which are eſſential to its formation. *1ſt.* An oxidable ſubſtance, one end or ſide of which *only* is to be oxidated; *2d,* A ſubſtance from which the electric fluid generated during the oxidation can abſtract hidrogen: theſe two ſubſtances muſt be alternately arranged, either perpendicularly or horizontally; the firſt ſets the fluid at liberty, the ſecond confines it and conducts it in the proper direction. When we admit the neceſſity of the oxidation of the zinc, we immediately ſee the reaſon why the pile ceaſes to act when perfectly pure water is interpoſed between the plates, why the action is ſuſpended in vacuo and increaſed by oxigen gas. The hypotheſis advanced above ſhews us how an apparatus may be conſtructed of one metal and charcoal, of one metal alone, or even of charcoal alone, provided a proper fluid be interpoſed to each ſurface. The experiment related by Mr. Davy, where, upon uniting the ends of the pile to two glaſſes of water by means of muſcular fibres, while the glaſſes themſelves were connected by a ſilver wire, the effects of the ends of the wire were reverſed, admits of a ſimple ſolution. The fluid muſt have been conducted along the fibres ſolely in conſequence of the moiſture attached to them; when it left the plus end of the pile it would therefore decompoſe part of this water, and paſs along in union with hidrogen to the ſilver wire, where the hidrogen would be diſengaged. The other end of the ſilver will then become oxidated, and the hidrogen abſorbed will be evolved at the minus end of the pile.

I have protracted this communication to ſo unuſual a length, that I ſhall only add one additional remark with reſpect to the fact firſt aſcertained by Fourcroy, that the power of the pile in giving ſhocks is not proportionate to its power in conſuming metals. A few alternations of large plates are more powerful in effecting the latter operation, while the ſame quantity of metal employed in conſtructing a higher pile of leſs diameter, affords a more powerful ſhock. It appears from every circumſtance, that the force of the ſhock depends almoſt entirely upon the concentration of the electric fluid; the ſame quantity which, when contained in a few jars highly charged, would be ſufficient to deſtroy animal life, if diſperſed over 100 jars would be ſcarcely perceptible. On the other hand, in the combuſtion of metals the abſolute quantity of electricity is of great importance; provided the current be rapid (which, from the experiments of Dr. Van Marum, appears to be the caſe with the pile of Volta in an amazing degree), and if the quantity of fluid be conſiderable, its ſtate of intenſity is of leſs moment *. The intelligent reader will at once perceive in what way the fact obſerved by Fourcroy admits of explanation from this train of reaſoning.

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* Cuthbertſon firſt obſerved in a letter to me (1793) the ſtriking fact, that the combuſtion of equal lengths of the ſame wire is made by equal quantities of electricity, whether the intenſity be greater or leſs (within certain extended limits). See Account of Experiments. Philoſ. Journal, quarto, II. 215. Auguſt, 1798.—N.

III.

On the ſuppoſed Exiſtence of Mechanical Depoſits and Petrefactions in the Primitive Mountains, and an Account of Petrefactions which have been diſcovered in the neweſt Flötz Trappformation.

By Mr. ROBERT JAMESON. *Communicated by the Author.*

The primitive rocks precipitated from a great depth of water which covered the globe.

OUR globe, according to the Wernerian geognoſia, even during the depoſition of the newer primitive ſtrata, appears to have been covered to a great height with water, as is evinced by the want of all mechanical depoſit *. After the precipitation of theſe great rock formations, the level of the water became ſo low, as to allow it to act mechanically upon the ſubjacent rocks; this occaſioned the firſt mechanical depoſition, which diſcovers itſelf in the tranſition rocks (Ubergangsgebürge). Nearly at the ſame time organization commenced, as it is in the tranſition rocks we find the firſt traces of organic remains: theſe are generally zoophytes and ſea plants, a fact which goes deep not only into geology, but natural hiſtory.

The tranſition rocks when the water was lower; at which time organization began.

Profeſſor Playfair, in his illuſtrations of the Huttonian theory, mentions ſeveral inſtances of mechanical depoſits and petrefactions which have been diſcovered among the primary ſtrata, and from theſe he concludes, that no ſuch ſeries of ſtrata as the tranſition exiſt. I ſhall now examine the ſtatements he has given; and firſt reſpecting the occurrence of petrefactions in primary mountains.

Inſtances of mechanical depoſits in primary ſtrata; offered by Profeſſor Playfair.

At page 164 he obſerves, "Another ſpot, affording inſtances of ſhells in primitive limeſtone, is in Devonſhire, on the ſea ſhore, on the eaſt ſide of Plymouth dock, oppoſite to Stonehouſe, I found a ſpecimen of ſhiſtoſe micaceous limeſtone, containing a ſhell of the bivalve kind; it was ſtruck off from the ſolid rock, and cannot poſſibly be conſidered as an adventitious foſſil. Now, no rocks can be more decidedly primary than thoſe about Plymouth; they conſiſt of calcareous ſtrata, in the form either of marble or micaceous limeſtone, alternating with ſhiſtus of the ſame kind, which prevails through Cornwall to the weſt, and extends eaſtward into Dartmoor, and on the ſea coaſt as far as Berry Head. Theſe all inter-

Shells in primitive limeſtone, in the vicinity of Plymouth, &c.

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* The exception to this in the ſienite formation I explained in a former paper.

ſect

But it is not proved that thoſe ſtrata are primitive.

Whence the concluſion of the Profeſſor is not agreed to.

ſect the horizontal plane in a line from eaſt to weſt nearly; they are very erect, thoſe at Plymouth being elevated to the north." That petrefactions exiſt in the limeſtone at Plymouth is evident; but that theſe ſtrata are primitive, ſtill remain to be proved. The character given of the limeſtone does not exclude it from the tranſition ſtrata; but of the ſhiſtus we cannot judge, as neither its oryctognoſtical, or geognoſtical characters are given. The other inſtances which are alluded to, are liable to the ſame objection. I cannot therefore agree with Profeſſor Playfair in believing "Though, therefore, the remains of marine animals are not frequent among the primary rocks, they are not excluded from them; and hence the exiſtence of ſhell fiſh and zoophytes, is clearly proved to be anterior to the formation even of thoſe parts of the preſent land which are juſtly accounted the moſt ancient."

The poſition that vegetable matters are found in primitive ſtrata need not be examined.

Profeſſor Playfair agrees with Dr. Hutton in affirming, that vegetable matters occur in the primitive ſtrata: I do not find it neceſſary to enter into an examination of what they have ſaid upon this ſubject, as they have evidently confounded a geognoſtical with an oryctognoſtical inveſtigation.

Prof. Playfair's proofs of mechanical depoſits in primitive ſtrata examined.

I ſhall now examine the proofs which Profeſſor Playfair has brought to eſtabliſh the exiſtence of mechanical depoſits in primitive ſtrata.

Mount St. Gothard a central primitive mountain, has arenaceous ſtrata in its vicinity.

The firſt we meet with is from Sauſſure. Profeſſor Playfair remarks, " St. Gothard is a central point, in one of the greateſt tracts of primary mountains on the face of the earth, yet arenaceous ſtrata are found in its vicinity. Between Ayrolo and the Hoſpice of St. Gothard, Sauſſure found a rock, compoſed of an arenaceous or granular paſte, including in it hornblende and garnets. He is ſomewhat unwilling to give the name gres to this ſtone, which Mr. Beſſon has done; but he nevertheleſs deſcribes it as having a granulated ſtructure."

Obſervation. The rock here mentioned was gneiſs or mica ſlate.

The rock of Ayrolo is primitive, and is either gneiſs or mica ſlate. Garnets are ſeldom found in gneiſs, but are characteriſtic for mica ſlate; the geognoſt, therefore, would not heſitate to conſider the rock here mentioned as belonging to mica ſlate.

Inſtances by P. Playfair of indurated arenaceous granite.

Profeſſor Playfair continues, " Among the moſt indurated rocks that compoſe the mountains of this iſland, many are arenaceous. Thus, on the weſtern coaſt of Scotland, the great body of high and rugged mountains on the ſhores of Araſaig,

&c.

&c. from Ardnamurchan to Glenelg, conſiſts, in a great meaſure, of a granitic ſandſtone, in vertical beds. This ſtone ſometimes occupies great tracts; at other times, it is alternated with the micaceous, and other varieties of primary ſhiſtus; it occurs, likewiſe, in ſeveral of the iſlands, and is a foſſil which we hardly find deſcribed or named by writers on mineralogy."

Obſ. This granitic ſandſtone is gneiſs.

The granitic ſandſtone of Glen-elg is moſt certainly gneiſs *, and a variety which is not uncommon; and I may venture to ſay, that the ſtrata in Araſaig, &c. are of the ſame nature.

Other examples of Pr. Playfair. Granulated quartz in beds alternating with the ſhiſtus.

Profeſſor Playfair concludes with ſtating the following examples, as a further confirmation of his opinion—" Much alſo of a highly indurated, but granulated quartz, is found in ſeveral places in Scotland, in beds of ſtrata, alternated with the common ſhiſtus of the mountain. Remarkable inſtances of this may be ſeen on the north ſide of the ferry of Balachuliſh, and again on the ſea ſhore at Cullen. At the latter, the ſtrata are remarkably regular, alternating with different ſpecies of ſhiſtus. At the former, the quartz is ſo pure, that the ſtone has been miſtaken for marble.

" Theſe examples are perhaps ſufficient; but I muſt add, that in the micaceous and talcoſe ſchiſti themſelves, thin layers of ſand are often found interpoſed between the layers of mica or talc. I have ſeen a ſpecimen from the ſummit of one of the higheſt of the Grampian mountains, where the thin plates, of a talcy or aſbeſtine ſubſtance, are ſeparated by layers of a very fine quartzy ſand, not much conſolidated. The mountain from which it was brought, conſiſts of vertical ſtrata, much interſected by quartz veins. It is impoſſible to doubt, in this inſtance, that the thin plates of the one ſubſtance, and the ſmall grains of the other, were depoſited together at the bottom of the ſea, and that they were alike produced from the degradation of rocks more ancient than any which now exiſt."

Whence he concludes that they were depoſited (mechanically) at the ſame time.

Obſ. Granular quartz is not a mechanical, but a chemical depoſition. For this differs from mica ſlate in the abſence of ſlaty fracture; but the ſeries

I am ſurprized Profeſſor Playfair ſhould adduce granular quartz as a proof of mechanical depoſition, as it has no more claim to ſuch a character than granite. The following obſervations will render this evident; granular quartz differs from mica ſlate, in the abſence of the ſlaty fracture, and mica; we

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* Mineralogy of the Scottiſh iſles, vol. ii. p. 160.

have,

between each are gradual.

Mica ſlate differs from gneiſs in wanting felſpar; but here alſo the tranſition is equally gradual.

And gneiſs differs from granite by its ſlaty fracture, which it as gradually loſes.

The gradation from granular quartz to granite being perfect, the one is no more a mechanical depoſit than the other.

have, however, a ſeries from the moſt complete granular quartz to the moſt perfect mica ſlate. Again, mica ſlate, which differs from gneiſs, in wanting felſpar, is to be obſerved in all the intermediate ſtages until it paſſes into complete gneiſs; the gneiſs, which is principally diſtinguiſhed from granite by its ſlaty fracture, gradually loſes this fracture, and at length is not to be diſtinguiſhed from granite. Thus we have a complete gradation from the pureſt granular quartz to granite, and not only in hand ſpecimens, but in the mountains themſelves. It therefore follows, that if granular quartz is a mechanical depoſit, ſo is granite, a poſition which I believe the Huttonian ſyſtem would not allow.

Looſe texture is no proof of mechanical depoſition;

and layers of ſand are found in veins which in the volcanic theory are ſaid to bear every mark of complete fuſion.

That the granular quartz ſhould occur ſometimes of a very looſe texture; nay, even as Profeſſor Playfair remarks ſandy, is not ſurprizing, for in granite and baſalt we have ſimilar appearances. Even in veins, which to uſe Profeſſor Playfair's own words, bear all the marks of complete fuſion, layers of granular quartz, from the moſt compact to the looſeneſs of ſand have been obſerved. The great ſand veins in the Hartz afford remarkable inſtances of this; alſo, as I have more lately diſcovered, the lead veins at Wanlock-head in Lancaſhire.

Account of Organic remains which have been diſcovered in the neweſt Flötz Trapp formation.

General obſervations.

Many different accounts had been given of the geognoſtic relations of the rocks of this formation, before they engaged the attention of Werner, the great founder of *true geognoſia*. After having made the remarkable diſcovery upon the hill of Scheibenberg, he extended his inquiries to all the baſalt hills in Germany, and found in every quarter correſponding appearances. This confirmed more completely the concluſions he had then drawn, and intirely overthrew the volcanic ſyſtem. He however did not ſtop here; his after obſervations diſcloſed a connection among theſe appearances, which at firſt he was not probably aware of; they placed the Neptunian ſyſtem beyond the reach of attack, and completely annihilated a hoſt of hypotheſes. He proved,

Reſults. 1. That this is the neweſt of the great rocks.

2. That all the now unconnec-

1. That this is the neweſt of all the great rock formations, of which the cruſt of the earth is compoſed.

2. That all the apparently unconnected hills and maſſes of this formation, have formerly ſtood in connection with each other.

3. That it exiſts in all quarters of the globe.

ted maſſes of this formation were once connected.

Theſe facts lead him to the great diſcovery, that this formation at one time, extended as a cover around the whole earth. From theſe obſervations it is evident, that we may expect to meet with veſtiges of many of the organic and unorganic matter which at that time exiſted upon the cruſt of the earth, in the rocks of this formation. Accordingly, the inveſtigations of geognoſts have diſcovered organic remains in every rock of this formation; the following are given as inſtances:

3. That it exiſts every where.
4. That it formerly covered the whole earth, and muſt contain organic remains.

Organic remains in Greenſtone, Baſalt, Wacke, and Trapp Buccin.

QUADRUPEDS.

1. Werner in his geognoſtic lectures, informs us that the wacken of Kalten-nordheim is ſometimes found to contain deers horns.

Organic remains in greenſtone, baſalt, &c. found; viz. of quadrupeds.

2. The Abbe Fortis diſcovered the head of an unknown animal in a ſoft wacken, in the valley of Ronca in the Veroneſe. *Vide Beſchrebbung des Thales Ronca, ſ.* 96.

3. Sauſſure obſerved bones of quadrupeds in the wacken of the catacombs of Rome.

Lettre a Mr. le Chev. Hamilton, J. de Phyſique. Tom. VII.

SHELLS AND ZOOPHYTES.

1. Dr. Richardſon found ſhells in rocks of the Trapp formation at Ballycaſtle in Ireland.—*Kirwan's Geol. Eſſays.*

Of ſhells and zoophytes.

2. Mr. Von Buch informs us, that in the county of Landeck, he obſerved a bed of wacken, which contained beſides pebbles of chalcedony, turbinites in a ſtate of complete preſervation. *Verſuch einer mineralogiſten Veſchreibung von Landeck, von Leopold von Buch. ſ.* 35.

3. Abbé Fortis obſerved numerous petrefactions of ſhells in Wacken, and Trapp Breccia, alſo a few in Baſalt, in the formation of the valley of Ronca.

4. Berolding found a cornu ammonis, which ſtill retained its mother of pearl luſtre, in the baſalt of Torez. In the baſalt of Thurgau, near the Boden lake, he obſerved gryphites, ammonites, and gloſſopetræ, *Chem. Annal.* 1794, p. 103.

5. In the Wernerian collection of petrefactions I ſaw ſpecimens of greenſtone, containing petrefactions of ſhells.

Of vegetables.

VEGETABLE REMAINS.

1. Werner obſerved great trees, with branches, leaves, and fruit, in the wacken, at Joachimſthal.

2. Frieſleben deſcribes the impreſſion of a plant in the Kawſower Berg, near to Podſedlitz.

3. In the iſlands of Banna and Skye I obſerved pieces of wood in Trapp Breccia, and Mineralogy of the Scottiſh iſles, vol. ii. p. 58—75.

Organic remains in ſlaty clay, limeſtone, and ſandſtone, of this formation.

Organic remains found in ſlaty clay, limeſtone, and ſandſtone, which belongs to the Flötz Trapp formation.

Shells.

SHELLS.

1. Abbé Fortis, in his account of the valley of Ronca, informs us, that limeſtone and ſlaty clay often alternate with baſalt, wacken, and trapp breccia. The limeſtone and ſlaty clay contains numerous petrefactions of ſhells, which are of the ſame kinds with thoſe found in the baſalt, wacken, and trapp breccia.

2. In the iſland of Eigg, where there appears to be a ſimilar formation with that of Ronca, I obſerved that the limeſtone, ſlaty clay, and ſandſtone, contained numerous petrefactions of ſhells. Mineralogy of the Scottiſh iſles, vol. ii. p. .

Of vegetables.

VEGETABLE REMAINS.

1. At the northern extremity of the iſland of Skye, where baſalt alternates with limeſtone and ſlaty clay, I obſerved pieces of carbonated wood in the limeſtone. Mineralogy of the Scottiſh iſles, vol. ii. p. 80[*].

2. In the flinty ſandſtone, which uſually accompanies this formation, I have obſerved branches of ſhrubs; vegetable matters that occur inveloped in rocks are generally, either carbonated or bituminated, here however they are not altered.

R. JAMESON,

Sheriff Brœ, Leith.

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[*] Mr. Kirwan, in the firſt volume of his Syſtem of Mineralogy, has given us an excellent account of the different opinions reſpecting the formation of baſalt; and has adduced many arguments that ſhew the fallacy of the Volcanic and Plutonic hypotheſis.

POSTSCRIPT.

POSTSCRIPT.

Since writing the incloſed, I have examined one of thoſe appearances, which are conſidered by Dr. Hutton and Profeſſor Playfair, as demonſtrating the exiſtence of petrefactions in primitive mountains. Dr. Hutton, at p. 334 of his Theory of the Earth, remarks, " I have already obſerved, that one ſingle example of a ſhell, or of its print, in a ſchiſtus, or in a ſtone ſtratified among thoſe vertical or erected maſſes, ſuffices to prove the origin of theſe bodies to have been, what I had maintained them to be, water formed ſtrata created from the bottom of the ſea, like every other conſolidated ſtratum of the earth. But now, I think, I may affirm that there is not, or rarely, any conſiderable extent of country of the primary kind, in which ſome mark of this origin will not be found, upon careful examination; and now I will give my reaſon for this aſſertion. I have been examining the ſouth alpine country of Scotland occaſionally, for forty years back, and I could not find any mark of an organized body in the ſchiſtus of thoſe mountains. It is true, that I knew of only one place where limeſtone is found among the ſtrata: this is upon Tweedſide near the Crook. This quarry I had carefully examined long ago, but could find no mark of any organized body in it. I ſuppoſe they are now working ſome other of the vertical ſtrata near to thoſe which I had examined: for, in the ſummer of 1792, I received a letter from Sir James Hall, which I ſhall now tranſcribe. It is dated Moffat, June 2, 1792.

Dr. Hutton's ſtatement of facts to ſhew the mechanical origin of primitive countries from the exiſtence of organic remains.

" As I was riding yeſterday between Noble Houſe and the Crook, on the road to this place, I fell in with a quarry of alpine limeſtone; it conſiſts of four or five ſtrata, about three feet thick, one of them ſingle, and the reſt contiguous; they all ſtand between the ſtrata of ſlate and ſchiſt, that are at that place nearly vertical. In the neighbourhood, a ſlate quarry is worked of pure blue ſlate; ſeveral of the ſtrata of ſlate near the limeſtone, are filled with fragments of limeſtone ſcattered about like the fragments of ſchiſt in the ſandſtone, in the neighbourhood of the junction on our coaſt. Among the maſſes of limeſtone lately broken off for uſe, and having the fracture freſh, I found the forms of cockles quite diſtinct, and in great abundance. I ſend you three pieces of this kind," &c.

Sir James Hall's account of organic remains in a limeſtone ſtated to be primitive.

It may perhaps be alledged, that thoſe mountains of Cumberland and Tweedale are not the primary mountains, but compoſed of the ſecondary ſchiſtus, which is every where known to contain theſe objects belonging to a former earth. Naturaliſts who have not an opportunity of convincing themſelves by their proper examination, muſt judge with regard to that geological fact by the deſcription of others. Now it is moſt fortunate for natural hiſtory, that it has been in this range of mountains that we have diſcovered thoſe marks of a marine origin; for, I ſhall afterwards have occaſion to give the cleareſt light into this ſubject, from obſervations made in other parts of thoſe ſame mountains of ſchiſt, by which it will be proved that they are primary ſtrata; and thus no manner of doubt will remain in the minds of naturaliſts, who might otherwiſe ſuſpect that we were deceiving ourſelves, by miſtaking the ſecondary for the primitive ſchiſtus.

Remark that the limeſtone is not primitive,

Dr. Hutton's account of the mountains in the ſouth of Scotland is confuſed and unſcientifical, and hardly comes within the pale of *true geognoſia*. It is not my intention, at preſent, to enter into an examination of his obſervations; the object of this poſtſcript is to ſhew, that the limeſtone between Noble Houſe and the Crook Inn does not belong to the primitive mountains. The beds of limeſtone mentioned by Sir James Hall, I obſerved lying between ſtrata of tranſition ſlate, and this ſlate alternating with ſtrata of grey wacke; conſequently the whole belongs to the tranſition claſs of rocks.

for it lies between ſtrata of tranſition ſlate; alternating with ſtrata of grey wacke.

Deſcription of the ſeveral rocks.

The limeſtone has a blueiſh grey colour, fracture is foliated, the diſtinct concretions are from coarſe to fine grained, and it is hardly tranſlucid on the edges. It is often traverſed by veins of calcareous ſpar, and ſometimes it contains thin beds of flinty ſlate (Kieſel Schiefer of Werner). The tranſition ſlate has a blueiſh or ſmoke grey colour, has generally leſs luſtre than the primitive ſlate, and contains much interſperſed mica. I obſerved it in all the ſtages from nearly pure ſlate to grey wacke. The grey wacke is compoſed of fragments of tranſition ſlate, flinty ſlate, and quartz, connected by a baſis of tranſition ſlate. It is frequently traverſed by veins of quartz, and is to be obſerved where the fragments are hardly diſtinguiſhable from their ſize; it has much the appearance of a breccia. I ſhall take another opportunity of ſending you drawings of the different kinds of petrefactions that occur in the limeſtone.

IV. *A*

IV.

A Method of increaſing the Senſibility of the Barometer, ad libitum. By the Rev. James Wilson, *A. M. Communicated by the Author.*

THOUGH there have been many contrivances for making barometers with indefinite ſcales, they all ſeem to labour under ſome difficulty either in their conſtruction or uſe.

The evaporation of water in that of Des Cartes, the ſluggiſh motion of the mercury in the horizontal and diagonal ones, the friction in Dr. Hooke's, and the unſteadineſs of floating bodies in Rownings, have rendered theſe ingenious contrivances ſcarce preferable to the common vertical one.

Contrivances to enlarge the ſcale of the barometer by water; by horizontal or inclined columns, by mechaniſm and a float &c. have not proved uſeful.

None of theſe defects, I believe, will attach to that which I propoſe. It is as follows: (See Fig. 1. Plate II.) A B, a tube differing in no reſpect from that of the common barometer, but that it is wider and longer, ſo that a cylindrical rod, *q r*, may float freely on the mercury, and that the lower end is connected to a ſmall bent tube B C D, inſtead of being immerſed in a ciſtern. To the lower end of the floating rod is faſtened a hair, or fine iron wire, (what I uſed was a piece of the Indian weed uſed by anglers) which is brought through the mercury, and out at D, ſo that the rod may be either drawn down, or ſuffered to aſcend at pleaſure.

Deſcription of a new barometer. A metallic rod floats in the mercury of a ſyphon barometer.

Upon the tube C D is a mark N, to which by moving the rod we can at any time bring the ſurface of the mercury. For by drawing it down we force up mercury into each tube, and on the other hand, by ſuffering it to aſcend, we ſuffer the mercury to deſcend in each.

The ſurface of the mercury then being brought to this mark, and the rod being faſtened, by tying the hair to the pin E, if on inſpecting it afterwards, we find that the ſurface of the mercury has moved from N, we are ſure there has been ſome variation in the weight of the atmoſphere, viz. if it has fallen, we know that this weight has increaſed, and *vice verſâ*.

and is drawn down into the mercury till the internal riſe cauſes the lower ſurface to ſtand at a fixed mark.

The variations either at M or N are in a given ratio to the variations in the length M N of the ſuſtained column. For the riſe or fall at M, is to the ſimultaneous fall or riſe at N, in the inverſe ratio of the baſes, or horizontal ſections of the mercury

The variations at the inner or outer ſurface are proportioned to thoſe of the whole column.

mercury at M and N; namely, as the area of the horizonta ſection of the tube D C is to the difference of the areas of the horizontal ſections of the tube A B, and of the rod. That is, if D, d, and r be the diameter of the tubes A B, D C, and rod, as $d^2 : \overline{D^2 - r^2}$, which is a given ratio, and conſequently each quantity has to the ſum of both, which is the variation in the length M N a given ratio, *e. gr.* d^2 (or $D^2 - r^2$) : $D^2 + d^2 - r^2$.

The variations either at M or N would therefore, if we could meaſure them with ſufficient accuracy, expreſs truly the variation in the weight of the atmoſphere.

and ſo are thoſe in the riſe or fall of the rod; if it be in every caſe adjuſted to the mark.

But if inſtead of attempting this, we move the rod by means of the hair until the mercury return to N, by the length of rod moved either into the mercury, or out of it, we have another meaſure, and one as long as we pleaſe, of the variations in the weight of the atmoſphere.

And this meaſure may be enlarged at will, Proof.

For, ſuppoſing the mercury to have fallen at N, any ſpace N x, and riſen at M, a ſpace M y, for the adjuſtment we muſt draw down the rod until it has diſplaced as much mercury as will fill the ſpace N x, and alſo a ſpace y x in the other tube of preciſely the ſame bulk, through which it muſt riſe at the ſame time. The length of rod drawn down for each of theſe purpoſes will have a given ratio to the ſpace N x, and conſequently their ſum added to M y will have a given ratio to it: and thence, as N x is a meaſure of the variation of the atmoſphere, this portion of rod will alſo be a meaſure: and manifeſtly it may be increaſed at pleaſure by leſſening the thickneſs of the rod.

This appears as follows. The proportion of the riſe in one tube to the ſimultaneous fall in the other upon any change of the weight of the atmoſphere, was above determined to be as $\frac{1}{D^2 - r^2} : \frac{1}{d^2}$. Let q be the *quantity* of mercury transferred from one tube to the other, the heights M y and N x will be $= \frac{1}{D^2 - r^2}$ and $\frac{q}{d^2}$ and the ſum of theſe is the variation which would take place in De Luc's barometer.

The variation M $y = \frac{q}{D^2 - r^2}$ is ſeen on the rod without moving it; upon moving it there is added to this, firſt, $\frac{q}{r^2}$

to occaſion the aſcent N x (the bulk of rod drawn down for that purpoſe being = to the bulk of q, and thence its altitude $= \frac{q}{r^2}$) and ſecondly, $\frac{q\,D^2 - q\,r^2}{d^2\,r^2}$ to occaſion the equal ſimultaneous aſcent $y\,x$ (ſince the bulk of rod drawn for that purpoſe is greater than the bulk q in the ratio $D^2 - r^2 : d^2$, and therefore equal to $\frac{q\,D^2 - q\,r^2}{d^2}$ and its altitude equal to $\frac{q\,D^2 - q\,r^2}{d^2\,r^2}$).

The ſum of theſe three $\frac{q}{D^2\,r^2} + \frac{q}{r^2} + \frac{q\,D^2 - q\,r^2}{d^2\,r^2}$ is the whole variation ſeen on the rod. This is to $\frac{q}{D^2 - r^2} + \frac{q}{d^2}$, the variation on De Luc's ſcale, (by bringing them to a common denominator) as $D^4 - 2\,D^2\,r^2 + D^2\,d^2 + r^4 : D^2\,r^2 + d^2\,r^2 - r^4$, which is a given ratio; and therefore the rod affords an accurate meaſure.

Let s and 1 be the rod ſcale, and the common ſcale $s = \frac{D^4 - 2\,D^2\,r^2 + D^2\,d^2 + r^4}{D^2\,r^2 + d^2\,r^2 - r^4}$, which by performing the diviſion gives the infinite ſeries $\frac{D^2}{r^2} - 1 + \frac{d^2}{D^2} - \frac{d^4}{D^4} +$ &c. $\frac{D^2}{r^2}$ is the principal term of this quote; for the ſum of the infinite ſeries commencing with -1 is $\frac{-D^2 + r^2}{D^2 + d^2 - r^2}$ which is neceſſarily leſs than a negative unit; and if d be ſuppoſed to vary, decreaſes ſtill as it increaſes.

The ſcale therefore being increaſed in ſo trifling a degree by the increaſe of d, may be ſaid to be as $\frac{D^2}{r^2}$.

The narrower the tube D C is I ſhould ſuppoſe the better; ſince though the ſcale is diminiſhed in a ſmall degree, yet the variation N x is rendered more ſenſible, which is a point of great importance.

Injury from agitation is thus alſo in ſome degree prevented.

The ſcale of th barometer beſt obtained from obſervation.

Though we can thus from D, d, and r calculate the ſcale, yet as we can ſcarcely meaſure them with ſufficient accuracy, the beſt way is to determine it by obſervation, viz. by finding how much of the rod expreſſes ſome larger variation in the common barometer, and then taking out the rod and graduating, &c. or rather by taking the mean of a great number of obſervations.

Should this be found on a ſufficient trial to anſwer the purpoſe, I can point out a method of making it ſerve alſo as a thermometer.

This barometer was ſuggeſted by reflection on another conſtruction.

Before I conclude, I ſhall juſt mention the idea which led me to this conſtruction, which though the greateſt increaſe of ſcale it gives is 14 : 1, may poſſibly in the end be found to anſwer better.

Water poured on the expoſed ſurface of a ſyphon mercurial barometer gives a barometer which may be made to ſhew variations 14 times as long as the mercurial change.

It ſtruck me that if we were to pour in water into a long tube D F, connected with the tube of the common barometer as in Fig. 2, or to take out water ſo as at any time to bring the mercury to the fixed point N, the altitude of water either poured in or taken out would give us a meaſure for the variations of the barometer fourteen times larger than in the common one, ſince water is fourteen times lighter than mercury. This is evident from the common hydroſtatical principles, as the altitude of water poured in is to be a counterpoiſe to the ſum of the fall of mercury in A B, and riſe in C D, and *e contra*, the altitude of water taken out is to be a counterpoiſe to the ſum of the riſe of mercury in A B, and fall in C D.

If the tube D F therefore were graduated, it would give a ſcale 42 inches long, and the riſe of water would indicate diminution in the weight of the atmoſphere and *v. v.* thence the ſcale to be inverted. To facilitate the pouring out and in of water, let there be a bucket H moveable up and down by rackwork, or otherwiſe, from D to F; to this let there be attached the ſyphon S.

By raiſing the bucket until the level of the water in it be higher than the level of that in the tube, we pour in water at pleaſure, and *e contra*, by bringing down the bucket we take out water at pleaſure.

This inſtrument will have all the advantages of a water barometer without its unmanageable length; nor do I ſee any objection to it, except perhaps that its appendages are too bulky *.

V. *A Summary*

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* The principal objection to the ingenious contrivances in the preſent communication are, that the reſults originally depend on the

V.

A Summary of the moſt uſeful Parts of Hydraulics, chiefly extracted and abridged from Eytelwein's Handbuch der Mechanik und der Hydraulik. Berlin, 1801. *By* THOMAS YOUNG, *M. D. F. R. S.*[*]

State of hydraulic theory, and difficulties of applying it to practice.

THE theory of hydraulics has never been carried to a very high degree of perfection upon mathematical foundations alone; nor has it hitherto, even with the aſſiſtance of experiment, been rendered of much practical utility. Newton began the inveſtigation of the motions of fluids: Daniel Bernoulli added to Newton's propoſitions much valuable matter, both from calculation and from experiment; D'Alembert and many later authors have exerciſed their analytical talents in inquiries of a ſimilar nature. But another and a more practicable mode of attaining hydraulic knowledge has been attempted by a diſtinct claſs of inveſtigators, at the head of whom ſtands the Chevalier de Buat. Theſe have begun from experiment alone, and have laboriouſly deduced from very ample obſervations of the actual reſults of various particular caſes, the general laws by which the phenomena appear to be regulated, or at leaſt the formulas by which the effect of new combinations may be predicted. But it muſt be confeſſed that theſe formulas, however accurate, are too intricate to be retained in the memory, or to be very eaſily applied to calculations from particular data.

Mr. Eytelwein's valuable compendium.

Mr. Eytelwein, a gentleman already known to the public by his tranſlation of Buat's work into German, with important additions of his own, and honoured with ſeveral employments and titles relative to the public architecture of the Pruſſian dominions, has collected into this compendium of mechanics and hydraulics, the principal facts that have been aſcertained, as well by his own experiments, as by thoſe of former authors,

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the accuracy of adjuſtment at N, in which the errors are preciſely the ſame as in the common barometer. The double barometer affords an inſtance of a lighter fluid floating on a larger; but with a ſomewhat ſhorter ſcale than that of fig. 2.—N.

[*] This excellent memoir is copied from the Journals of the Royal Inſtitution.

eſpecially ſuch as are the moſt capable of practical application; and he appears to have done this in ſo judicious a manner, as to make his book a moſt valuable abſtract of every thing that can be deduced from theory reſpecting natural and artificial hydraulics. This elegant conciſeneſs deſerves ſo much the more praiſe, as his countrymen in general appear too often to make a merit of prolixity; and we ſhall have occaſion to remark, that beſides the convenience of ſimplicity, he has ſometimes been fortunate enough to unite with it the advantage of ſuperior accuracy.

Part 1. Mechanics.

The firſt part of the work is but ſhort; it relates to proper mechanics, and has little that is remarkably new or intereſting. In treating of pendulums, the author informs us, with reference to another work of his own, that the Rhinland or Brandenburg foot contains 139.13 French lines. Hence it appears that 100 Rhinland feet are exactly 103 Engliſh; and in this paper, the meaſures will be reduced accordingly.

Part 2. Hydraulics.

The ſecond part, relative to hydraulics, contains beſides a ſhort introduction, twenty-four chapters, almoſt every one of which preſents to us ſomething of importance.

Water iſſuing from reſervoirs.

Chapter 1. Of the motion of water flowing out of reſervoirs, and of the contraction of the ſtream.

Law of its velocity: As the ſquare root of the height.

§ 89. The velocity of water flowing out of a horizontal aperture, is as the ſquare root of the height of the head of water.

That is, the preſſure, and conſequently the height, is as the ſquare of the velocity: for the quantity flowing out in any ſhort time, is as the velocity; and the force required to produce a velocity in a certain quantity of matter in a given time, is alſo as that velocity; therefore the force muſt be as the ſquare of the velocity. The propoſition is fully confirmed by Boſſut's experiments; the proportional velocities, with a preſſure of 1, 4, and 9 feet, being 2722, 5436, and 8135, inſtead of 2722, 5444, and 8166; a very inconſiderable difference.

Another developement.

There is another mode of conſidering this propoſition, not mentioned by Eytelwein, which is a very good approximation. Suppoſing a very ſmall cylindrical plate of water immediately over the orifice, to be put in motion at each inſtant by means of the preſſure of the whole cylinder ſtanding on it, and ſuppoſing all the gravitation of the column to be employed in generating the velocity of the ſmall cylindrical plate, neglecting its own motion, this plate would be urged by a force as much greater than its own weight as the column is higher than itſelf; and this, through a ſpace ſhorter in the ſame proportion than the height of the column. But where the forces are inverſely as the ſpaces deſcribed, the final velocities are equal. (Young's Syllabus, 35.) Therefore the velocity of the water flowing out muſt be equal to that of a heavy body falling from the height of the head of water, which is found very nearly by multiplying the ſquare root of that height in feet by 8, for the number of feet deſcribed in a ſecond. Thus a head of 1 foot gives 8; a head of 9 feet, 24.

The well-known circumſtance of the contraction of the ſtream or vein of water running out of a ſimple orifice in a thin plate, reduces the area of its ſection at the diſtance of about half its diameter from the orifice, from 1 to .660 or .666 according to Boſſut, to .631 according to Venturi, and to .64 or $\frac{16}{25}$ according to the author's own experiments: hence the diameter is reduced to $\frac{4}{5}$.

The contraction of the ſtream.

The quantity of water diſcharged is very nearly, but not quite, ſufficient to fill this ſection with the velocity due, or correſponding to the height: for finding more accurately the quantity diſcharged, the orifice muſt be ſuppoſed to be diminiſhed to .619, or nearly $\frac{5}{8}$. Hence we may multiply the ſquare root of the height by 5 inſtead of 8, for this mean velocity in a ſimple orifice.

Practical rule for a ſimple orifice. Multiply the ſquare root of the height by 5 gives the velocity in 1 ſecond.

If we apply the ſhorteſt pipe that will cauſe the ſtream to adhere every where to its ſides, which will require its length to be twice its diameter, the diſcharge will be about $\frac{13}{16}$ of the full quantity, and the velocity may be found by taking $6\frac{1}{2}$ for a multiplier.

Additional pipe in length twice the diameter of the hole requires a multiplier of $6\frac{1}{2}$.

The greateſt diminution is produced by inſerting a pipe ſo as to project within the reſervoir, probably becauſe of the greater interference of the motions of the particles approaching its orifice in all directions: in this caſe the diſcharge is reduced nearly to a half.

Diſadvantage of an interior pipe;

A conical tube, approaching to the figure of the contraction of the ſtream, procured a diſcharge af .92; and when its edges were rounded off, of .98, calculating on its leaſt ſection.

conical, &c. produced a diſcharge of 0.98.

Venturi* has aſſerted that the diſcharge of a cylindrical pipe may

Venturi's conical tube;

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* Venturi's treatiſe is given entire in our Journal, 4to, Vol. II. and III.

may be increaſed by the addition of a conical tube nearly in the ratio of 5 to 2; but Mr. Eytelwein finds this aſſertion ſomewhat too ſtrong, and obſerves, that when the pipe is already very long, ſcarcely any effect is produced by the addition of ſuch a tube. He proceeds to deſcribe a number of experiments made with different pipes, where the ſtandard of compariſon is the time of filling a given veſſel out of a large reſervoir, which was not kept always full, as it was difficult to avoid agitation in repleniſhing it, and this circumſtance was perfectly indifferent to the reſults of the experiments. They confirm the aſſertion that a compound conical pipe may increaſe the diſcharge to twice and a half as much as through a ſimple orifice, or to more than half as much more as would fill the whole ſection with the velocity due to the height: but where a conſiderable length of pipe intervenes, the additional orifice appears to have little or no effect.

increaſes the diſcharge from a ſimple aperture twice and a half.

The chapter concludes with a general table of the coefficients for finding the mean velocity of the water diſcharged by the preſſure of a given head under different circumſtances.

Multipliers for determining the velocity under different circumſtances.

For the whole velocity due to the height, the coefficient, by which its ſquare root is to be multiplied, is 8.0458.

For an orifice of the form of the contracted ſtream, 7.8.

For wide openings, of which the bottom is on a level with that of the reſervoir; for ſluices with walls in a line with the orifice; for bridges with pointed piers, 7.7.

For narrow openings, of which the bottom is on a level with that of the reſervoir, for ſmaller openings in a ſluice with ſide walls, for abrupt projections and ſquare piers of bridges, 6.9.

For ſhort pipes from two to four times as long as their diameter, 6.6.

For openings in ſluices without ſide walls, 5.1.

For orifices in a thin plate, 5.

Chapter 2. Of the diſcharge of water by horizontal and by ſmall lateral orifices, in a veſſel continuing full.

Particular caſes.

The principles detailed in the firſt chapter are here applied to particular caſes.

Diſcharge through a notch in the dam,

Chapter 3. Of the diſcharge by rectangular orifices in the ſide of a reſervoir, extending to the ſurface.

The velocity varying nearly as the ſquare root of the height, may here be repreſented by the ordinates of a parabola, and the quantity of water diſcharged by the area of the parabola, or two thirds of that of the circumſcribing rectangle. So that the quantity of water diſcharged may be found by taking two-thirds of the velocity due to the mean height, and allowing for the contraction according to the form of the opening, as explained in the firſt chapter.

has nearly two-thirds of the velocity due to the mean height.

The author has found this mode of calculation ſufficiently near to the reſults of Buat's experiments, and to ſome accurate obſervations of his own.

He propoſes for example a lake in which a rectangular opening is made without any oblique lateral walls, 3 feet wide, and extending 2 feet below the ſurface of the water. Here the coefficient of the velocity, corrected for contraction, is 5.1, and the corrected mean velocity $\frac{2}{3}\sqrt{2}\times 5.1=4.8$; therefore the area being 6, the diſcharge of water in a ſecond is 28.8 cubic feet, or nearly four hogsheads.

Particular exemplification.

The ſame coefficient ſerves for determining the diſcharge over a were of conſiderable breadth; and hence it is eaſy to deduce the depth or breadth requiſite for the diſcharge of a given quantity of water. For example, a lake has a were 3 feet in breadth, and the ſurface of the water ſtands at the height of 5 feet above it: it is required how much the were muſt be widened in order that the water may be a foot lower. Here the velocity is $\frac{2}{3}\sqrt{5}\times 5.1$, and the quantity of water $\frac{2}{3}\sqrt{5}\times 5.1\times 3\times 5$; but the velocity muſt be reduced to $\frac{2}{3}\sqrt{4}\times 5.1$, and then the ſection will be $\frac{\frac{2}{3}\sqrt{5}\times 5.1\times 3\times 5}{\frac{2}{3}\sqrt{4}\times 5.1}=\frac{\sqrt{5}\times 3\times 5}{\sqrt{4}}=7.5.\sqrt{5}$; and the height being 4, the breadth muſt be $\frac{7.5}{4}\sqrt{5}=4.19$ feet.

Chapter 4. Of the diſcharge from reſervoirs with lateral orifices of conſiderable magnitude, with a conſtant head of water.

This may be found by determining the difference in the diſcharge by two open orifices of different heights: but in moſt caſes the problem may be ſolved with nearly equal accuracy, by conſidering the velocity due to the diſtance of the centre of gravity of the orifice below the ſurface.

By lateral orifices of conſiderable magnitude, the velocity due to the center of gravity of the orifice may be taken.

Chapter 5. Of the diſcharge from reſervoirs receiving no ſupply of water.

For

Reſervoirs not ſupplied.

For priſmatic veſſels, all the particulars of the diſcharge may be calculated from the general law that twice as much would be diſcharged from the ſame orifice if the veſſel were kept full during the time which is required for its emptying itſelf. (Young's Syllabus, 245). Where the form is leſs ſimple, the calculations become intricate, and are of little importance.

Divided reſervoirs.

Chapter 6. Of the diſcharge from compound or divided reſervoirs.

The author obſerves from Buat, that the diſcharge through an orifice between two reſervoirs, below the ſurface, is the ſame as if the water ran into the open air. Hence he calculates the diſcharge when the water has to paſs through ſeveral orifices in the ſides of as many reſervoirs open above. In ſuch caſes, where the orifices are ſmall, the velocity in each may be conſidered as generated by the difference of the heights in the two contiguous reſervoirs, and the ſquare root of the difference will therefore repreſent the velocity; which muſt be in the ſeveral orifices, inverſely as their reſpective areas; ſo that we may calculate from hence the heights in the different reſervoirs when the orifices are given. Mr. Eytelwein then conſiders the caſe of a lock which is filled from a canal of an invariable height, and determines the time required, by comparing it with that of a veſſel emptying itſelf by the preſſure of the water that it contains, obſerving that the motion is retarded in both caſes in a ſimilar manner; and he finds the calculation agree ſufficiently well with experiments made on a large ſcale. The motion of water through different compartments of a cloſed cavity is alſo determined.

A lock filled from a canal.

Velocity of a river conſidered.

Chapter 7. Of the motion of water in rivers.

The ſimple theorem by which the velocity of a river is determined, appears to be the moſt valuable of M. Eytelwein's improvements; although the reaſoning from which it is deduced is ſomewhat exceptionable. The friction is nearly as the ſquare of the velocity; not becauſe a number of particles proportional to the velocity are torn aſunder in a time proportionally ſhort, for according to the analogy of ſolid bodies, no more force is deſtroyed by friction when the motion is rapid than when ſlow, but becauſe when a body is moving in lines of a given curvature, the deflecting forces are as the ſquares of the velocities (Young's Syllabus, 40, 35, 46), and the particles of water in contact with the ſides and bottom muſt be deflected, in conſequence of the minute irregularities of the ſurfaces on which they ſlide, nearly into the ſame curvilinear path, whatever their velocity may be.

Elementary poſition, and inductions.

At any rate we may ſafely ſet out with the hypotheſis, that the principal part of the friction is as the ſquare of the velocity. And the friction is nearly the ſame at all depths: for Profeſſor Robiſon found that the time of the oſcillation of the fluid in a bent tube was not increaſed by increaſing the preſſure againſt the ſides, being nearly the ſame when the principal part of the tube was ſituated horizontally as when vertically. The friction will however vary according to the ſurface of the fluid which is in contact with the ſolid, in proportion to the whole quantity of fluid: that is, the friction for any given quantity of water will be as the ſurface of the bottom and ſides of a river directly, and as the whole quantity of water in the river inverſely: or ſuppoſing the whole quantity of water to be ſpread on a horizontal ſurface, equal to the bottom and ſides, the friction is inverſely as the height at which the river would then ſtand, which is called the hydraulic mean depth.

Now when a river flows with an uniform motion, and is neither accelerated nor retarded by the action of gravitation, it is obvious that the whole weight of the water muſt be employed in overcoming this friction; and if the inclination vary, the relative weight or the force that urges the particles along the inclined plane, will vary as the height of the plane when the length is given, or as the fall in a given diſtance (Young's Syllabus, 54); conſequently the friction, which is equal to the relative weight, muſt vary as the fall, and the velocity which is as the ſquare root of the friction, muſt be as the ſquare root of the fall; and ſuppoſing the hydraulic mean depth to be increaſed or diminiſhed, the inclination remaining the ſame, the friction would be diminiſhed or increaſed in the ſame ratio; and therefore in order to preſerve its equality with the relative weight, it muſt be proportionally increaſed or diminiſhed by increaſing the ſquare of the velocity in the ratio of the hydraulic mean depth, or the velocity in the ratio of its ſquare root.

The velocities will be as a mean proportional between the hydraulic mean depth and the fall;

We may therefore expect that the velocities will be conjointly as the ſquare root of the hydraulic mean depth and of the fall in a given diſtance, or as a mean proportional between theſe two lines. Taking two Engliſh miles for a given length, we muſt find a mean proportional between the hydraulic mean depth and the fall in two miles, and inquire what relation this bears to the velocity in a particular caſe, and thence we may expect to determine it in any other. According to Mr. Eytelwein's formula, this mean proportional is $\frac{11}{10}$ of the velocity in a ſecond.

or 11-10ths of the velocity per ſecond.

Confirmation by a practical inſtance.

In order to examine the accuracy of this rule, we may take an example which could not have been known to Mr. Eytelwein. Mr. Watt obſerved, as Profeſſor Robiſon informs us in the article River of the Encyclopædia Britannica, that in a canal 18 feet wide above, and 7 below, and 4 feet deep, having a fall of 4 inches in a mile, the velocity was 17 inches in a ſecond at the ſurface, 14 in the middle, and 10 at the bottom: ſo that the mean velocity may be called 14 inches, or ſomewhat leſs, in a ſecond. Now to find the hydraulic mean depth, we muſt divide the area of the ſection, $2. (18+7)=50$ by the breadth of the bottom and length of the ſloping ſides added together, whence we have $\frac{50}{20.6,}$ or 29.13 inches: and the fall in two miles being 8 inches, we have $\surd(8\times29.13)=15.26$ for the mean proportional, of which $\frac{10}{11}$ is 13.9; agreeing exactly with Mr. Watt's obſervation. Profeſſor Robiſon has deduced from Buat's elaborate theorems 12.568 inches for the velocity, which is conſiderably leſs accurate.

Another inſtance.

For another example we may take the Po, which falls 1 foot in two miles, where its mean depth is 29 feet; and its velocity is obſerved to be about 55 inches in a ſecond. Our rule gives 58, which is perhaps as near as the degree of accuracy of the data will allow.

On the whole, we have ample reaſon to be ſatisfied with the unexpected coincidence of ſo ſimple a theorem with obſervation: and in order to find the velocity of a river from its fall, or the fall from its velocity, we have only to recollect that the velocity in a ſecond is $\frac{10}{11}$ of a mean proportional between the hydraulic mean depth and the fall in two Engliſh miles. This is however only true of a ſtraight river flowing through an equable channel.

True only in a ſtrait river, &c.

Slope of the banks.

For the ſlope of the banks of a river or canal, Mr. Eytelwein recommends that the breadth at the bottom ſhould be $\frac{2}{3}$ of the depth, and at the ſurface $\frac{10}{3}$: the banks will then be in general capable of retaining their form. The area of ſuch a ſection is twice the ſquare of the depth, and the hydraulic mean depth $\frac{2}{3}$ of the actual depth. He then inveſtigates the diſcharge of a canal of which the bottom is horizontal. The velocity appears in this caſe to be ſomewhat greater than in a ſimilar canal, of which the bottom is parallel to the ſurface.

Velocities whe the channel is curved

The author remarks that the velocity is greater near the concave than the convex ſide of a flexure: a circumſtance probably occaſioned by the centrifugal force accumulating the water on that ſide. No general rule can be given for the decreaſe of the velocity in going downwards: but ſometimes the maximum appears to be a little below the ſurface. In the Arno the velocities are at 2 feet below the ſurface, 39½ inches; at 4, 38½; at 8, 37; at 16, 33½; at 17, 31. In the Rhine at 1 foot, 58 inches; at 5, 56; at 10, 52; at 15, 43. As an approximation to the mean velocity, the author directs us to deduct from the ſuperficial velocity $\frac{1}{110}$ for every foot of the whole depth. For inſtance; if the depth were 13 feet, and the ſuperficial velocity 5 feet, to take 4½ as the average velocity of the whole river. This can however only be true in large rivers: for in the canal, meaſured by Mr. Watt, the ſuperficial velocity muſt be diminiſhed nearly $\frac{1}{5}$ for a depth of only 4 feet. And we may in general come quite as near to the mean velocity by taking $\frac{9}{10}$ of the ſuperficial velocity; although this may ſtill differ materially from the true medium. But comparing this with the former theorem for the velocity, which gives a reſult oftener above than below the truth, we may bring them both into a form eaſily recollected, thus;

at different depths.

Conciſe deduction of the hydraulic mean depth.

The ſuperficial velocity of a river is nearly a mean proportional between the hydraulic mean depth and the fall in two miles; and the mean velocity of the whole water is, ſtill more nearly, nine-tenths of this mean proportional.

We may find a double confirmation of theſe principles in Major Rennel's account of the Ganges (Phil. Tranſ. 1781, p. 87).

Inſtance in the Ganges.

He informs us that "at 500 miles from the ſea, the channel is 30 feet deep when the river is at its loweſt; and it continues at leaſt this depth to the ſea," that "a ſection of the ground, parallel to one of its branches, in length 60 miles, was taken by order of Mr. Haſtings;" that "the windings of the river were ſo great as to reduce the declivity on which the water ran, to leſs than 4 inches per mile;" that "the medium rate of motion of the Ganges is leſs than three miles an hour in the dry months;" that is, its ſuperficial velocity. Now allowing a little for the banks or ſhelving ſides, we may take exactly 30 feet as the hydraulic mean depth; then if the fall in two miles were preciſely $\frac{2}{3}$, we ſhould have $\frac{2}{3} \times 30 = 20$; and $\sqrt{20} = 4.47$ for the velocity in a ſecond, or 3.05 miles in the hour: which is a little greater than the obſerved velocity, becauſe the fall was aſſumed ſomewhat too great.

Again (p. 110), "the river when full, has thrice the volume of water in it, and its motion is alſo accelerated in the proportion of 5 to 3. We may aſſume, that the hydraulic mean depth is doubled at the time of the inundation, whence the velocity will be increaſed in the ratio of 7 to 5: but the inclination of the ſurface is probably ſomewhat increaſed at the ſame time, which may eaſily be ſuppoſed to increaſe the velocity ſtill further, from 1.4 to 1.7.

The effects of weres, falls, and contractions in rivers.

Chapter 8. Of the diſcharge and the ſwell in the caſe of weres, falls, and contractions, in rivers and canals.

The methods employed in the third chapter require here ſome modification, ſince the water arrives at the place of deſcent with a conſiderable velocity; and it is evident from mechanical, as well as from hydraulic conſiderations, that the ultimate velocity will exceed that which is due to the depth of the ſtream at the place of its deſcent, and that it will correſpond to a height equal to the ſum of the heights capable of producing theſe velocities. Hence we may calculate the effect of a bar in elevating the ſurface of a river; how broad a were muſt be, in order to produce a certain elevation, and how much water will run over a given were according to collateral circumſtances. When a bar is below the level of the lower water, we muſt conſider the difference of the two levels as conſtituting the fall; the whole of the ſtream below the level of the lower water deriving its additional velocity from this difference only.

The extent of the ſwell produced by a given elevation of the ſurface of a river in conſequence of the effect of a were or bar, may be determined by calculating from the rules for finding the velocity of rivers, the inclination neceſſary for producing a given diſcharge; the depth being greater, the inclination immediately above the bar will be leſs, but the effect of the ſwell does not terminate at the point where the new ſurface,

if

if ſtraight, would have met the original ſurface; for on account of the rounding off of this angle, it extends nearly twice as far. The effect of reducing the breadth of a river may be determined in a manner nearly ſimilar. The author remarks, that a conſiderable diminution of breadth produces but a ſmall elevation, a reſult which appears to be conformable to experience; but that where depth is required for navigation, it may often be obtained by a projection built out from the bank, which may be ſufficient to increaſe the river's velocity, and to cauſe it to excavate its bed.

(To be concluded.)

VI.

Experimental Proof, that Corrections deduced from the Arcs of Vibration of a Pendulum in Vacuo, are practically uſeful. By Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

SIR, *Lynn, Aug.* 20, 1802.

Introduction.

YOUR annotation on my laſt paper * convinces me, that I ought to have given you my reaſons, derived either from theory or experience, for troubling you on the ſubject which it contains.

Arguments might be offered in favour of applying the theory of pendulums to clocks;

Though theory may be too uncertain a guide to be depended on, in a matter ſo complicated as the vibration of a pendulum in a variable reſiſting medium, yet I think that ſome philoſophical arguments might be advanced in its favour; but theſe I ſhall not at preſent enter into, as I mean to confine myſelf to mere matters of experience.

but at preſent the author confines himſelf to experiment or fact.

The clock which I uſed formerly was made by the late Mr. John Arnold, in his beſt manner. It had a dead beat, with a compound pendulum that vibrated in the arc of a circle. This pendulum frequently varied in its arc; and as the ſhort vibrations were performed in leſs time than the long ones, I was led to ſuppoſe that it might be governed by the ſame law, which

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* See Vol. III. p. 273.

Clock of Arnold's make with dead beat and compound pendulum, found to vary according to the theory.

obtains in a pendulum vibrating in a non-reſiſting medium by the force of gravity: and, on making uſe of corrections deduced from that principle, I found them agree ſo well with the going of the clock, as left me no reaſon to doubt of the truth of my conjecture. The table which I ſent you was therefore conſtantly uſed to correct the clock's rate, when any alteration was obſerved in the arc of vibration; but the rate itſelf was aſcertained by tranſits of the ſun and fixed ſtars over the meridian.

It is thought that the corrections would not be applicable in ſome other combinations.

How far this table may be applicable to other clocks I know not, as this is the only inſtance in which I have had any experience. It is evident, however, that this theory cannot be uſed to advantage where a pendulum is acted upon either by cycloidal checks, a ſaddle-piece, or pallats of a particular conſtruction.

I am, SIR,
With much reſpect,
Your humble ſervant,
E. WALKER.

VII.

Deſcription of an Apparatus of Tubes for facilitating to driving of Copper Bolts into Ships. By Mr. RICHARD PHILLIPS[*].

Mr. Phillips's method of driving copper bolts, &c.

MR. RICHARD PHILLIPS, of Briſtol, in ſeveral letters ſent to the Society, ſtates, that he had invented a method of driving copper bolts into ſhips, without ſplitting the heads or bending them; and that by means of tubes contrived by him for the purpoſe, this could be effected without difficulty, and had been ſatisfactorily executed in the preſence of ſeveral of the principal ſhip-builders of Briſtol.

A certificate accompanied theſe letters, from Mr. William James, and Mr. Samuel Haſt, ſhip-builders, and alſo from Mr. George Winter, of Briſtol, teſtifying that they had tried

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[*] From the Tranſactions of the Society for the Encouragement of Arts, &c. for 1801. The Society adjudged a reward of forty guineas to the inventor. Models are in their repoſitory.

Evidence of its utility.

the experiment of driving copper bolts through the jointed cylinder invented by Mr. Phillips; and that they ſo far approve of it, that they mean to adopt the general uſe of them, for driving bolts in all directions, particularly on the outſide of ſhips, whether iron or copper; as this method not only prevents the bolts from bending, but keeps the heads from ſplitting, and enables the bolts to be driven much tighter, than by any other means with which they were acquainted. They further add, that by the application of Mr. Phillips's cylinder and punch, a copper bolt which had been crippled at the edge of the hole, and which could not be ſtarted by a mall, went up with eaſe in a perpendicular direction in the flat of a ſhip's bottom, not four feet from the ground.

This certificate was witneſſed by Mr. William Holden.

The ſame facts are alſo certified by Mr. Thomas Walker, and Mr. James M. Hillhouſe, of Briſtol, who add their opinion, that the adoption of this invention in the different dock-yards of the kingdom, will prove very advantageous.

Since Mr. Phillips's firſt application to the Society for a premium, he has made a conſiderable improvement in the conſtruction of his tubes. The deſcription and engraving hereunto annexed are of the improved kind: models of both are, however, preſerved in the Society's repoſitory, for public inſpection.

Deſcription.

The inſtrument employed for driving the bolts, conſiſts of a hollow tube formed from ſeparate pieces of caſt iron, which are placed upon the heads of each other, and firmly held thereto by iron circles or rings over the joints of the tubes. The loweſt ring is pointed, to keep the tube ſteady upon the wood. The bolt being entered into the end of the hole bored in the wood of the ſhip, and completely covered by the iron tube, is driven forward within the cylinder by an iron or ſteel punch, placed againſt the head of the bolt, which punch is ſtruck by a mall: and as the bolt goes further into the wood, parts of the tubes are unſcrewed and taken off, till the bolt is driven home into its place up to the head.

The tubes are about five inches in circumference, and will admit a bolt of ſeven-eighths of an inch in diameter.

Reference

Reference to the Engraving of Mr. R. Phillips's Method of driving Bolts into Ships. Plate III. Fig. 3, 4, 5, 6.

Fig. 3.

A. The copper bolt, with one end entered into the wood, previous to fixing the tube.

B. A piece of timber, or ſhip's ſide, into which the bolt is intended to be driven.

Fig. 4.

CCCC. The parts of the iron tube faſtened together, ready to be put on the bolt A.

DDDDD. Iron or braſs rings with thumb-ſcrews, placed over the joints of the tube, to hold them firm together.

EEEEE. The thumb-ſcrews, which keep the rings and tubes firm in their proper places.

F. Two points formed on the lower ring: they are to ſtick into the timber, and to enable the tube to be held firm in its place.

Fig. 5.

Shows the ſeparation of the parts of the tube, which is effected by ſlackening the thumb-ſcrews and rings.

To put them together, you ſlide the rings over the joints, placed as cloſe as poſſible; then, by tightening the thumb-ſcrews, you will have them firm together, and may continue the tubes to any length, from one foot to whatever number is required.

Fig. 6.

GH. Two ſteel punches or drifts, to be placed on the head of the copper bolt within the tube, whilſt driving. The blow given upon the punch drives forward the bolt. The ſhorteſt of them ſhould be uſed firſt, and, when driven nearly to its head, ſhould be taken out of the tube, and the longer punch applied in its place.

VIII.

Reply to Dr. YOUNG'S *Letter on the Theory of Compound Sounds. In a Letter from Mr.* JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

On the proper ſpirit of contro-verſy.

Explanatory re-marks.

That compoun ſounds are mix-tures,

and not forme by coaleſcence,

YOU have publiſhed Dr. Young's anſwer to my remarks on the theory of compound ſounds; and I doubt not but you will, with equal impartiality, find a place for the following reply in your Journal. When a controverſy is conducted with good humour, it affords amuſement; and when it furniſhes freſh facts, or places the ſubject of diſpute in a new point of view, it proves inſtructive. Dr. Young's anſwer poſſeſſes both theſe qualities; and it is my duty to imitate him, at leaſt in the obſervance of the former. Having now completed what may be called the introduction of the preſent letter, I will, with the permiſſion of Mr. Nicholſon, make a few remarks upon the principal objections contained in the anſwer under conſideration. The Doctor obſerves, that my theory of vibrations in an elaſtic fluid differs as widely from Dr. Smith's hypotheſis as from his own conceptions relating to the ſame operations: but he will probably recollect, that I profeſs to maintain compound ſounds to be mixtures of elementary ſounds, not aggregates by coaleſcence; in other words, I undertake to defend Dr. Smith's propoſition, as a fundamental maxim both of harmonics and the general philoſophy of ſounds. When the queſtion is thus properly ſtated, the engagement I undertake to perform cannot in any ſenſe oblige me to ſupport the collateral arguments, with which Dr. Smith thought proper to elucidate his propoſition, perhaps unfortunately. For if the poſitive maxim, *viz.* that compound ſounds are mixtures, be eſtabliſhed, the deſign of my eſſay is accompliſhed: and as Dr. Young does not controvert the leading concluſions of this paper, I may fairly ſuppoſe he admits them, but am at a loſs to underſtand in what manner they favour his ſide of the queſtion; for if my inferences be juſt, compound ſounds are not formed by coaleſcence, which is the point in debate. I do not pretend to ſay preciſely what would be the conſequence of conducting a number of ſounds

Fact reſpecting bells, of which the ſounds are ſaid not to coaleſce.

through a narrow tube, becauſe I do not recollect a circumſtance of the kind; nor do I think myſelf bound to determine the effect, becauſe an inſtance, which is ſo particular, can have no weight in a diſpute concerning a general principle. I am acquainted, however, with one circumſtance which deſerves attention at preſent: the notes of two or more bells are not obliged to coaleſce by paſſing through the narrow ſound-holes of the ſteeple of a country church: and if Dr. Young will ſuppoſe the point A, in the demonſtration of my third propoſition, to be placed in the mouth of his tube, he will ſee that the motions of the corpuſcle will not be diſturbed by the edge of the pipe. The Doctor's anſwer blames my repreſentation of his idea of a compound ſound, as amounting to a charge of ignorance in the moſt common occurrences. The repreſentation appeared to be a neceſſary introduction to the objections which were levelled, in the ſequel of my eſſay, at the Doctor's theory, not at his experience; and if the ſtatement be not juſt, it is difficult to diſcover the ſenſe of the term coaleſcence in that ſection of his paper in which the ſubject is examined, eſpecially when the author infers, that the ſtrength of the ſound in a concert is not in exact proportion to the number of inſtruments compoſing it. The writer of this paragraph, without doubt, wiſhed his readers to conclude, that the vibrations, which are communicated to the air by a number of ſounding bodies, reduce themſelves to a ſingle ſet of vibrations by mutual oppoſition: Now my objections to this doctrine muſt remain in force, until mankind can be convinced that unity poſſeſſes the qualities of number.

On the mixture of ſounds differ-ng in their ranges.

The anſwer corrects the new theory in two eſſential points: *Firſt,* It gives a power of analyzing compounds to the ear; but this power cannot be admitted before the doctrine of coaleſcence is eſtabliſhed: *Second,* It reduces the coaleſced compound to a pigmy, and joins to it the powerful reflected parts of its conſtituents, thereby forming a mixture. A few remarks on the range of ſuch a mixture ſhall cloſe this letter. When a traveller approaches a town, he frequently hears the bells in it at the diſtance of four or five miles; the beating of a drum may catch his attention at one-third of that diſtance; when near the place, he perceives the blows of hammers, and at laſt a mixed noiſe. In his journey, then, he meets with a ſucceſſion of ſounds at the limits of their reſpective ranges, but does not find the coaleſced compound in the van of the train. If the preceding ſtatement be natural, the range of a mixture of ſounds muſt be eſtimated by means of the ſtrongeſt individual in the aggregate, not from the coaleſced compound, admitting its exiſtence. This concluſion holds good in a general ſenſe; at the ſame time I know, that the ranges of certain aggregates exceed the ranges of their ſtrongeſt conſtituents. Of this kind is a general diſcharge of fire-arms; but though I admit the fact, it does not appear to be an inſtance of coaleſcence. For the report of every muſket employed is nearly alike; and the exploſion of the whole number, at the ſame moment, agitates a large field of air. Now I know, by obſervation, that when an extenſive ſurface is made to vibrate it ſounds to a great diſtance; for when the ſhear-men of Kendal beat the tenters on a calm morning, the ſtrokes may be heard for two miles, or more; though the ear of a by-ſtander is not much affected by them, the operation being performed by ſtriking the ſtretched cloth with a ſtick not thicker than a man's finger. Seeing then a number of ſounds which are nearly in uniſon, forms an aggregate of more power than any one of its conſtituents, it follows, that when a multiplicity of inſtruments contains a ſtrong combination of uniſons, that combination determines the range of the concert.

Sounds of aggregates heard farther than their loudeſt conſtituents, muſquetry.

Shearmen beating the tenters heard two miles.

JOHN GOUGH.

Middleſhaw, near Kendal, Auguſt 19, 1802.

IX.

A new Proceſs for Claying Sugars, propoſed by Cit. Hassel Lachenaie, *Chief Apothecary of the Military Hoſpitals of Guadaloupe to the Agents of the Conſuls of the French Republic in the Windward Iſlands..*

(Concluded from Vol. II. Page 190.*)*

THE third and laſt which I ſhall mention, though the leaſt of the inconveniencies of the proceſs of Citizens Boucherie, cannot be reconciled with the economy which we are obliged to uſe in the employment of our time; namely, the neceſſity of graining the ſugar before it is put into the caſes, &c.

Economy of time.

After

Proceſs of the author.

After having pointed out the principal difficulties and inconveniences which would reſult in the manipulation of the claying of ſugar, by the uſe of caſes conſtructed after the model of thoſe of Citizens Boucherie, nothing more remains to be done, than to ſhew how I have ſucceeded in overcoming theſe difficulties, by uſing caſes fabricated according to my own principles.

Conſtruction of the caſes or receptacles for claying ſugars, deſcribed by reference to the drawings.

My caſes are without a bottom; they are conſtructed of four boards, united in a ſquare by tenons and mortices, which are held cloſely together by wedges and pins, ſo as to form a ſquare veſſel, each ſide of which is three feet long, inſide meaſure. This caſe therefore preſents a ſurface of nine ſquare feet. The ſide *h* (ſee Plate XII. Vol. II.) is 18 inches high at its extremity *e*, and 16½ inches at its other extremity *f*. The ſide *l*, parallel to *h*, is cut in the ſame proportions. The height of the ſide *k* correſponds with that of the extremity *l*; and the ſide *i*, has the ſame height as the extremity *e*.

I place four caſes, of the conſtruction juſt deſcribed, upon a frame eight feet long, or plank, *m*, *m*, *m*, *m*, having a fall of three inches, and raiſed two feet above the ground *qq*. This board ſerves as their bottom. In the ſpace occupied by each caſe, it is perforated with twenty holes of an inch in diameter, placed in four rows: theſe holes, which I ſtop beneath with pins projecting eight inches above the bottom within, ſerve for the drainage, when, after the ſugar has cooled, theſe ſame pins, which perforate it almoſt to the half of its thickneſs, are drawn out.

For receiving the ſyrup, I place below the board ſeveral channels which convey it into a common trough intended to conduct it into a reſervoir. Thirty-two caſes diſpoſed in this manner upon eight boards, in my ſugar work, will ſupply the place of 900 forms, and as many pots.

It is evident that by thus inclining the board which ſerves as a bottom to the caſes, it was indiſpenſibly requiſite that I ſhould give the ſame inclination to the lower edges of the ſides, in order that this upper part might be at the ſame level; and this inclination was the more neceſſary, as it favours the running off of the ſyrup at the borders, where the caſes are in contact with the board, no leſs than is done by the holes with which it is perforated.

After deſcribing the conſtruction and diſpoſition of the caſes which I propoſe, I ſhall now point out the manner in which I uſe them.

Manufacturing proceſs.

In order to avoid very fatiguing tranſportations, in which much time would be loſt, I have eſtabliſhed a cooler in my ſugar-work, into which the boiled ſugar is conveyed by a gutter from the battery. Near this cooler I have placed two backs, each of the ſame capacity with one of my caſes, and theſe laſt are all of equal dimenſions.

MANIPULATION.

The ſugar firſt cooled in a back,

I draw off my ſugar by two batteries *; when theſe are joined together in the cooler †, I pour them into one of the backs where the ſugar cryſtallizes by cooling. This ſugar is ſtirred two or three times during the interval, until two other batteries have been poured into the refrigeratory: I then fill up my back, and with a paddle I mix well together the ſugar thus collected; after which I leave it at reſt till it acquires the conſiſtence it is ſuffered to aſſume, when intended to be manufactured into raw ſugar. In the interval other ſugar is made, to be poured in the ſame manner into the ſecond back.

then put into the caſes to drain,

As ſoon as the ſugar has acquired the before-mentioned conſiſtence, I uſe ſmall buckets for conveying it into one of my caſes, which I fill to the height of ſix lines below its margin. I then leave it till coated, at which period I draw out the pegs from the board, in order that the ſyrup may run out.

eaſily examined to aſcertain the time for claying.

On the next and the following days, one of the ſides of the caſes may be opened to obſerve how the drainage goes on; but it muſt immediately be replaced. This facility, which has the advantage of aſcertaining the moſt favourable moment for claying the ſugar, enables us alſo to aſcertain at pleaſure the

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* The battery (*batterie*) is the veſſel in which the boiling of the ſugar is terminated, and when two boilings are united in the refrigeratory, it is cuſtomary in our manufactories to call this proceſs *drawing off the ſugar by two batteries (tirer le ſucre par deux batteries, l'enformer par deux batteries)*. Theſe two batteries conſtitute the filling (*emplie*). The name of *battery*, which ſerves to deſignate this boiler, is given to it becauſe the ſugar is beaten with a paddle, or ſtirrer, whenever it tends to raiſe itſelf above the ſides.

† This is a large boiler which receives the boiled ſugar, and in which it is ſuffered to loſe its heat before it is drawn off.

effect of the claying. When, by examining the ſugar in this manner, we find that there does not remain more than about two inches diſcoloured by the ſyrup, it is the proper time for claying it.

Preparation for the claying.

To prepare the ſugar for receiving the clay, the very thin cruſt formed at its ſurface is firſt taken off, and is divided and ſpread out again upon this ſurface, which is pierced to the depth of ſix lines with the edge of the trowel, which is afterwards uſed for flattening and compreſſing the bottom. After this laſt operation is performed, it is clayed in the uſual manner.

Qualities of the clay.

The fat earth or clay which is generally uſed here for claying of ſugars, has in general the fault of retaining the water with which it is diluted too long, on which account it happens, that whilſt one part of this water filtrates into the ſugar, another, and that not the leaſt conſiderable, evaporates into the air.

Defect from tenuity.

This earth has alſo another fault, which is very prejudicial to the operation of the bleaching of the ſugar; namely, that of remaining for ſeveral days in part ſuſpended in the water, before it is intirely precipitated from it, which renders this fluid milky. Both theſe faults require to be corrected.

After ſome trials I ſucceeded in doing this completely: my proceſs is ſimple, eaſy to be executed, and requires neither expence nor loſs of time; I can even aſſert, that the water with which my earth thus prepared is diluted, depoſits nothing by filtrating into the ſugar, but that it ſeparates as limpid as the pureſt rain water. I ſend you ſome ſugar bleached by this means.

Advantages reſulting from the Uſe of my Caſes and Manipulation for the claying of Sugar.

FIRST ADVANTAGE.

With Regard to the Weight.

Statement of advantages in this new apparatus and proceſs.

By caſing the ſugar in the manner here deſcribed previous to ſubjecting it to the claying proceſs, I prevent the formation of the *fountain (la fontaine)* *, and of the thick cruſt with which every form preſents us. Now, not having to ſeparate the fountains

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* This is deſcribed at page .

Leſs cruſt on the ſurface.

fountains of the twenty-ſix forms which each of my caſes contains, the quantity of the ſugar of theſe ſame fountains remains therefore in the maſs. This maſs alſo ſuffers a much ſmaller loſs in the claying operation, than it would have done if this operation had been performed upon twenty-ſix ſeparate forms.

SECOND ADVANTAGE.

With Reſpect to the Regularity and Uniformity of the Grain.

The grain very uniform.

When ſugar is made to cryſtallize in our forms, the cryſtals that are ſucceſſively precipitated from it, diminiſh alſo ſucceſſively in volume in riſing towards the top of the form which, in its poſition, then repreſents an inverted cone. In order to avoid this inconvenience, it is cuſtomary to ſtir the ſugar as ſoon as its cruſt becomes ſolid. Nevertheleſs, a fountain is almoſt always formed, and for this reaſon: when the maſs contained in our large forms begins to cool, the conſiſtence which its ſtill liquid part aſſumes, in proportion as it loſes the caloric by which it was liquefied, reſiſting the force of attraction which tends to unite the cryſtalline particles ſymmetrically together, in order to conſtitute regular cryſtals, theſe particles are precipitated, and apply themſelves confuſedly together, ſo as to produce the ſolid cruſt which conſtitutes this fountain.

Cauſe why the grain is better.

The greater uniformity of cryſtallization in the maſs which my caſes contain, (though I have diſturbed it by agitating it ſeveral times), compared with what happens in the forms which have not been moved, and even in thoſe which have been, with equal care and dexterity, is obtained at the moment when I caſe my ſugar. The conſiſtence which it has acquired no longer permits its cryſtals to obey the laws of ſtatics; but the ſmaller being confounded with the larger, the whole preſents a perfect uniformity, not to be found in thoſe forms which have been moved. For in them this operation is performed at a moment when there ſtill exiſts a ſufficient degree of liquidity to permit the larger cryſtals to obey the ſame laws.

No ſyrup collected at the top.

The reaſon why no ſyrup is collected above the ſugar depoſited in my caſes, depends on the ſeparation of the cryſtals, which being not at all agglutinated together, leave intervals ſufficiently large to receive it. But as this ſyrup ſtill retains much of its heat when the ſugar is caſed, it depoſits a ſufficient number of cryſtalline particles in cooling to agglutinate the cryſtals, and to cauſe the whole maſs to ſettle by its own weight in proportion as the ſyrup runs off, and acquire a force of adheſion equal to that of the ſugar put into forms.

THIRD ADVANTAGE.

As to its Whiteneſs or Colour.

and much leſs at the baſe.

The caſed ſugar bleaches as well under the clay, as that which is put into forms, and its baſe retains proportionally leſs ſyrup than the head of the form.

FOURTH ADVANTAGE.

With Reſpect to the Manipulation.

Comparison. 1. *Leſs Labour and loſs of Time.*

Numerical ſtatement of the ſaving of labour.

When we fabricate ſugar, we employ every day a man to waſh the forms, to carry them to the ſugar houſe, and to plant them.	By the uſe of the caſes, I avoid the neceſſity for employing this man, and the loſ of this time.
For putting the ſugar into forms, two men carry it and divide it in the forms, which they fill. When theſe forms are ſufficiently cool, they carry them into the refining houſe, in order to put them upon pots into which their ſyrups drain.	For caſing the ſugar, it i tranſported, one ſingle time by ſmall buckets into my caſes which are cloſe to the backs This operation is much leſs fatiguing than that of the forms which weigh about 80 lbs. a the moment when they ar thus conveyed.
After ſome days drainage, the bottom of each form is deeply perforated, in order to extract the fountain which is ſeparated from it. When this buſineſs is finiſhed, the forms are again carried to other pots, deſtined to receive their fine ſyrup, after which the bottom is diſpoſed for the claying.	When, on opening one o the ſides of my caſes, the ſuga is found to be in the prope ſtate for claying, its bottom i prepared as I have deſcribed page , after which the diluted earth is poured upon it.
In treating 416 forms in this manner, four men generally work 51,711 hours, which, multiplied by 4, amounts to 206,844 hours work for one man.	In this manner four me clay 16 caſes, which contain a much as 416 forms, in th ſpace of 2,840 hours, whic multiplied by 4, amounts t 11,360 hours work for on man.

Thu

Thus the labour of claying 416 ſeparate forms, with reſpect to the time employed in doing it, is to that for the ſame number of forms contained in 16 caſes, as 206,844, to 11,360, or as 51,711 to 2,840. Now therefore, in order to clay the quantity of 416 forms of ſugar in my 16 caſes, (reckoning the day's work of each man at nine hours) I employ only one day and 2,360 hours, whereas the claying of 416 ſeparate forms requires 22 days and 8,844 hours.

The new method of claying performed in about one twentieth part of the uſual time.

I find alſo another ſaving of time and labour when the earth is to be removed and the bottom cleaned. This work, which requires a day's work of four men for the 416 forms, is performed in half a day by one man, upon the ſame quantity of ſugar in the caſes. I have made this compariſon with my watch in my hand.

2. *Fewer Difficulties in making the Bottom.*

Though our cultivators are in the habit of working the forms and making their bottom, it frequently happens that they neglect to remove the fountain completely; frequently alſo they do not uniformly compreſs the grained ſugar which they had taken out of the form, in order to ſeparate this fountain, and which they replace in it before making the bottom. In that caſe, the filtration of the water of the clay does not take place uniformly, but meets with obſtacles towards ſome of its ſides; and the bottom, after this claying, exhibits diſtinct marks of the defective manipulation.

In my caſes, as there are no fountains to be ſeparated, as the cryſtallization is uniform throughout the whole maſs, the ſurface of which is level before the cooling of the ſugar, it is ſufficient that this ſurface be very ſlightly opened before levelling the bottom: this buſineſs is very eaſily performed, and thoſe workmen to whom I have confided the execution for the firſt time, have perfectly ſucceeded. The uniformity of the cryſtallization in my caſes, alſo favours that of the filtration of the claying water into the maſs of the ſugar.

The bottom is levelled more eaſily.

3. *Another ſaving of Time and Labour.*

The forms being ſufficiently drained, the ſugar is taken out in order to be conveyed to the

When I find the ſugar ſufficiently drained in my caſes, I take them down, and leave them

The drying and ſtoving is performed with more facility and effect.

ſtove, where the loaves are depoſited upon frames, after the head, which is ſtill wet with ſyrup, has been ſeparated from them. For, without this precaution, it would flow back into the maſs, diminiſh its whiteneſs, and render the ſugar more liable to attract the moiſture of the atmoſphere. Some days afterwards theſe forms are cut in pieces, in order to complete the deſiccation of the ſugar, and to ſeparate the whiteſt parts from the more coloured. them for ſome days expoſed to the air, in order to deprive them of part of their humidity which is retained by that of the forms, when carried immediately to the ſtove: I afterwards break them into large pieces *, and at the ſame time I ſeparate the whiter ſugar from the reſt. By this means the workmen are ſaved the labour of dividing the ſame ſugar, as is commonly done, in a hot ſtove which frequently muſt not be ſuffered to cool without the danger of injuring the quality of the ſugar, and which, on account of the length of time they remain in it, diſpoſes them to contract dangerous diſeaſes.

FIFTH ADVANTAGE.

The laſt of the advantages which I ſhall mention, and which is not the leaſt important, is that of *economy*.

It is very expenſive, and ſometimes difficult to procure the forms and pots; but this apparatus is cheap and eaſily had.

It is known that it would coſt an immenſe ſum to thoſe who, like myſelf, being without forms and pots, are under the neceſſity of providing themſelves with them, ſince it is almoſt impoſſible to procure them, now that the Saints and Martinique are in the power of the enemy. With little expence, I conſtruct and eſtabliſh 32 caſes, which ſupply the place of 802 forms and as many pots, and by this means avoid the great loſſes occaſioned by the daily rupture of theſe veſſels.

I think it unneceſſary to recapitulate the obſervations contained in this memoir, and only wiſh that you may conſider them as having ſome title to your approbation.

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* Which are likewiſe carried into the ſtove, but which dry more ſpeedily than the loaves.

X. *Deſcription*

X.

Deſcription of a Water Wheel. By Mr. J. BESANT. *In a Letter to Mr.* CHARLES TAYLOR, *Secretary to the Society for the Encouragement of Arts. From their Tranſactions.*

SIR,

I BEG leave to lay before the Society ſome obſervations reſpecting the common Underſhot Water-wheel, and to point out the ſuperiority of that of my invention.

The common underſhot wheel

Firſt,—In common water-wheels more than half the water paſſes from the gate through the wheel, without giving it any aſſiſtance.

lets much water paſs without effect.

Secondly,—The floats coming out of the tail-water are reſiſted with almoſt the whole weight of the atmoſphere, at the inſtant they leave the ſurface of the water.

The floats are reſiſted at their riſe,

Thirdly,—The ſame quantity of water which paſſed between the floats at the head, muſt of courſe paſs between them at the tail, and conſequently impede the motion of the wheel.

and they tend to lift the tail water.

In the water-wheel of my invention,

Firſt,—No water can paſs but what acts, with all its force, on the extremity of the wheel.

In the new wheel

Secondly,—The floats coming out of the water in an oblique direction, prevent the weight of the atmoſphere from taking any effect.

the floats are oblique.

Thirdly,—Although the new water-wheel is heavier than that on the old conſtruction, yet it runs lighter on its axis, the water having a tendency to float it.

It is in part buoyant;

Fourthly,—By experiments made with the models, proofs have been ſhown, that the new wheel has many advantages over the common wheel, and that, when it works in deep tail water, it will carry weights in proportion of three to one, ſo that it will be particularly ſerviceable for tide-mills.

and is good for tide mills.

I hope on trial, before the Society, my invention will prove ſucceſsful, and am,

Sir,
Your obedient Servant,
J. BESANT.

No. 26, *Brompton.*
To the SECRETARY of the
Society of Arts, &c.

Tried with ſucceſs by the Society.

Repeated experiments of the above invention were made by the Committee; from the reſult of which it appeared to poſſeſs ſome advantages over the common wheel, and to have a greater power of action.

Deſcription of the late Mr. Beſant's Water-wheel. Plate III. Fig. 1 & 2.

Deſcription of the engraving.

A. The body of the water-wheel, which is hollow in the form of a drum, and is ſo conſtructed as to be proof againſt the admiſſion of water within it.

B. The axis on which it turns.

C. The float boards, placed on the periphery of the wheel. Each board is obliquely fixed firm to the rim of it, and to the body of the drum.

D. The reſervoir, containing the water.

E. The penſtock, which regulates the quantity of water running to the wheel.

F. The current of water which has paſſed the wheel.

Fig. 2. Is a front view of the water-wheel, ſhewing the oblique direction in which the float boards C are placed on the face of the wheel.

XI.

Deſcription of an Hydrometer which gives the Specific Gravity by Inſpection, conſtructed by Mr. ATKINS.—W. N.

Improvements in Atkins's hydrometer.

The weights of different figures.

SINCE the publication of the deſcription of Atkins's hydrometer in the laſt Number, that artiſt has communicated to me two improvements in the ſame. The firſt conſiſts in making the four weights of different figures, namely, round, ſquare, triangular, and pentagonal; and he ſtamps the figure of the weight on the ſliding rule, oppoſite to every letter in the ſeries to which it belongs. By this contrivance, which indicates a conſiderable degree of ſagacity with regard to the practical requiſites of an inſtrument offered for general uſe, he renders it impoſſible for the revenue officer to miſtake one weight for another, or to take out his reſult at a wrong part of the ſliding rule.

The

Hydrometer having the ſp. gravity graduated on its ſtem.

The other alteration, though it will be highly acceptable to philoſophical men, and renders the inſtrument of more extenſive utility, and independent of the ſliding rule, ſeems not ſo clearly to be an amelioration with regard to its moſt frequent and daily uſe. Inſtead of the letters of the alphabet, the four faces of the ſtem carry graduations which at once point out the ſpecific gravity of the fluid according to the form now univerſally adopted; namely, by taking water as unity. Its figure is ſhown in Plate IV. Fig. 1. and it is provided with three weights; the inſtrument itſelf being uſed without a weight at the upper range of denſities.

Remarkable overſight of philoſophical chemiſts in noting ſpecific gravities by the degrees of ſome particular inſtrument.

Upon this occaſion I cannot avoid noticing the ſtrange overſight of philoſophers and others, who have expreſſed the denſities of acids and other fluids by certain graduations or degrees of inſtruments, ſuch as that of Baumé and others; the comparative values of which were either little known to the public, or ill determined; ſo that a very eſſential part of what we read in books of chemiſtry is to many readers void of meaning. This is the more remarkable, ſince the expreſſion of ſpecific gravities before mentioned has been long eſtabliſhed, and is quite as eaſy to be put upon an inſtrument as any other notation. It is now ſeven years ſince I urged this matter in my chemical dictionary at the article Hydrometer, and the evil ſtill remains. If my recollection be accurate, I think Briſſon made the ſame propoſal many years ago in the Memoirs of the French Academy. In the preſent inſtance, Mr. Atkins gains the advantage of ſimplifying his ſliding rule by leaving out the alphabet, ſo that the operator looks for the ſpecific gravity in the firſt inſtance, and upon the oppoſite lines he finds the ſtrength and concentration.

Long ago noticed by the author, and by Briſſon.

Advantages in ſimplifying Atkins's ſliding rule;

and rendering the inſtrument applicable to all tables, &c.

But ſtill more it is worthy of notice, that if he poſſeſſed the inſtrument only, or ſhould in preference be deſirous of referring to Gilpin's tables, or any other elementary courſe of experiments, he can proceed immediately from the inſtrument to the table without uſing the ſliding rule to determine his ſpecific gravity, as he muſt with the other.

I was at firſt very ſtrongly impreſſed with the facility of this inſtrument in giving a reſult which ordinarily demands to be computed, and I ſuppoſe the advantages above ſtated will ſecure it the preference with many. But upon reflection, I cannot but think the alphabetic inſtrument intitled to the preference

But the alphabetic hydrometer appears better adapted for difpatch and ſecurity againſt unſkilfulneſs.

ference for commercial purpoſes, eſpecially in the hands of men unuſed to make experiments, or to read the graduations of a rule. It is ſo much more eaſy to pitch upon a ſingle letter than to read a graduation by ſtroke, as well upon the inſtrument as the ſcale, that I apprehend the literal inſtrument, with the variouſly figured weights, will in general be attended with greater certainty in the hands of all but philoſophers. And for this reaſon the inventor is undoubtedly right in offering both to the public.

Cylindric bulb.

The cylindrical figure of the bulb affords the means of diminiſhing the ſize of the aſſay jar, and conſequently will demand a leſs quantity of the ſpirit to make the requiſite trials.

XI.

On the Air from Finery Cinder and Charcoal, with other Remarks on the Experiments and Obſervations of Mr. Cruickſhank. In a Letter from JOSEPH PRIESTLEY, *L. L. D. F. R. S. &c. &c.*

To Mr. NICHOLSON.

DEAR SIR, *Northumberland (America), Ap.* 16, 1802.

MR. CRUICKSHANK having added a ſupplement to the account of his experiments on the air from finery cinder and charcoal, in anſwer to my objections to the new theory from the properties of that kind of air, I have given more particular attention to it, and wiſh you to add the following obſervations to thoſe which I have already ſent you on the ſubject.

The air from finery cinder and from charcoal ſtated to differ from other denſe inflammable air only in the proportion of fixed air it affords by combuſtion.

When I firſt procured this kind of air, I was far from imagining that I had diſcovered any *new* ſpecies of air, eſſentially different from any other, ſo as to be entitled to a new appellation, but only another variety in the heavy inflammable air, which is known to be exceedingly various in different proceſſes, and even in the different ſtages of the ſame proceſs, all however agreeing in this, that when all fixed air is carefully waſhed out of them, more is found on the decompoſition of them when they are fired together with dephlogiſticated air.

And in this eſſential property the air from finery cinder and charcoal agrees with them all, differing only in the proportion of the fixed air procured in this manner. This, however, I now find muſt be called *the gazeous oxide of carbon*, while the others are called *hydro-carbonates;* that being ſaid to conſiſt of two parts oxigen to one of carbon, and the others to be a ſolution of carbon in hidrogen, or the light inflammable air.

It is inflammable as well as the hydro-carbonates, and cannot with any conſiſtency be called an oxide, becauſe it is combuſtible.

It cannot, however, be denied, that this gazeous oxide of carbon is *inflammable* as well as the hydro-carbonates, in the compoſition of which a portion of hidrogen (one of the component parts of water) is a neceſſary ingredient. This air, therefore, from finery cinder and charcoal, though called an *oxide*, muſt be eſſentially different from all the other oxides, none of which are combuſtible, being ſubſtances already ſaturated with oxigen. Thus iron is a combuſtible ſubſtance, ready to unite with oxigen when preſented to it in a proper temperature; but when it is ſaturated with oxigen, and therefore called *an oxide of iron*, it is no longer combuſtible. It muſt, therefore, as it appears to me, be an abſolute abandonment of one of the moſt fundamental principles of the new theory, to call the air from finery cinder and charcoal an oxide. If ſubſtances be combuſtible in proportion to their affinity to oxigen, and their conſequent readineſs to unite with it, this air, which is inflammable, muſt be of *this* claſs, and therefore the very reverſe of the oxides, which are ſaturated with oxigen, and incapable of receiving more.

Its oxigen has never been ſeparated or ſhewn by tranſpoſition, as happens with real oxides.

If this kind of air was a real oxide, it would appear to be ſo on the decompoſition of it; when, to make the reſult unexceptionable, the oxigen it contained would either take the form of dephlogiſticated air, or become a component part of ſome other ſubſtance into which oxigen was acknowledged to enter. But this has not been done. When it is decompoſed by being fired together with dephlogiſticated air, the fixed air which is then formed comes, I have no doubt, from the oxigen in the dephlogiſticated air, and the phlogiſton in this ſpecies of inflammable air; the ſame being the reſult, though not quite in the ſame degree, of firing the heavy inflammable air from charcoal and water, from oil, &c. &c. into which it is not pretended that any oxigen enters.

Mr. Cruickſhank's obſervation of the union of the oxigen in the dephlogiſticated marine acid air with inflammable air in the common temperature of the atmoſphere, is extremely curious but I do not ſee that any thing more can be inferred from it, than from the more rapid union of the ſame principles when inflammable and dephlogiſticated air are fired together.

Objection to Mr. Cruickſhank's theory of the reduction of finery cinder and charcoal.

Mr. Cruickſhank ſays, p. 201, that "the oxigen in the finery "cinder, uniting with the carbon in the charcoal, forms fixed "air; and that the metal being in the ſame proceſs revived, "decompoſes this fixed air, when it becomes again to a cer"tain degree oxigenated." But why ſhould not the finery cinder retain a part of its oxigen, rather than firſt part with it, and then take it again? Beſides, it is not true that after this proceſs the iron is in any degree oxigenated, for it is completely revived, being perfect iron; and that any fixed air is either formed or decompoſed in this proceſs is altogether conjectural, and for the reaſons that I have given cannot be admitted. For though it might be poſſible for oxigen in the finery cinder (ſuppoſing it to contain that principle) to be extracted from it by its affinity with the carbon in the charcoal, and that nothing ſhould enter in its place, the iron thus revived could not decompoſe the fixed air that would be formed by their union.

Not doubting but that Mr. Cruickſhank will give a candid attention to theſe obſervations,

I am,

DEAR SIR,

Your's ſincerely,

J. PRIESTLEY.

XII. *Deſcription*

XII.

Deſcription of Mr. READ'S *Pneumatic Apparatus. By a Correſpondent.*

To Mr. NICHOLSON.

SIR,

I BEG leave to recommend to your notice a very ſimple and cheap apparatus for diſplacing any elaſtic fluid out of a reſervoir by means of water. It is deſcribed in Dr. Beddoes's Conſiderations on the Medicinal Uſe of Factitious Airs, octavo, London, 1796; and I hope you will think with me, that it deſerves a corner in your reſpectable publication.

Notice of a very ſimple apparatus for the gaſes.

I is a funnel with its pipe or tube ſtopped at B, and perforated above and below with two or three ſmall holes. Round the tube A is ſoldered the tube D, left open at top. The tube G is made to circumſcribe the tube D, and to be ſoldered to the funnel at F, and likewiſe ſoldered at E.

Deſcription.

When water is poured into the funnel, and the cork K K inſerted into a bottle filled with gas, the water deſcends through the tube A till it arrives at the diviſion or ſection B; it then flows through the ſmall holes at C, and aſcends between the tubes A and B; flows over the top of D, and deſcends again between the tubes D and G, till it arrives at E (where it regains the tube through an opening above E), and thence into the bottle. As the water goes in, the air eſcapes between the tubes A and G, through the cock H, into the mouth-piece. The lower end of G is made to perforate a cork which is properly attached to it, and which ſecures it air-tight into the neck of a common bottle, as ſeen at fig. 3.

Effect.

The whole apparatus may be made for leſs than a guinea, and may be introduced to the bed-ſide of the patient, if neceſſary, by having a caſk filled inſtead of a bottle, and placed upon a chair. By turning the cock the patient may take an inſpiration whenever he pleaſes, the cock being thruſt into the ſpiggot hole.

It is very cheap.

I am, &c.

N. N.

SCIENTIFIC

SCIENTIFIC NEWS, &c.

Experiment of the Inviſible Girl.

Deſcription of the apparatus for the experiment of the Inviſible Girl.

MONS. Charles, brother to the well known philoſopher of that name, has for ſome time exhibited in Leiceſter-ſquare an experiment called the Inviſible Girl, which ſeems to include ſome principle of acouſtics either newly diſcovered, or not hitherto known to the extent there diſplayed. In the middle of a large lofty room in an old houſe, where from the appearance of the wainſcot, the poſition of ſome glaſs caſes of natural hiſtory that occupy one ſide, and from other circumſtances, there does not appear to be any ſituation for acouſtic tubes or reflectors—is fixed a wooden railing about four feet high, and five feet wide, incloſing a ſquare ſpace. From the four corner poſts of this railing there riſe painted wires, or rods, to an eminence of ten or twelve feet, (the cieling being much loftier) and from the upper ends of theſe hangs, by ſtrings reſembling the lines uſed with curtains, a ſquare glazed box, having a mahogany bottom nearly on a level with the top of the railing, and in the middle of this box is ſuſpended an hemiſphere (or larger portion) of tin or pewter, out of which proceed horizontally four tin trumpets at right angles to each other, and about eighteen inches long. The diſk or hemiſphere is covered with a portion of a quickſilvered glaſs globe which completes the ſphere, and cloſes it imperfectly, ſo as not to prevent the inner aperture of one of the trumpets from being ſeen through the place of inſertion. This whole apparatus is ſo far detached from all ſurrounding objects, that it can be ſwung about as far as the railing will allow; that is to ſay, the box can be ſwung by its four ſtrings, and the globe and trumpets by the ſtring that ſuſtains them in the box. The globe may be about ten inches in diameter. In all theſe dimenſions, which I do not ſuppoſe to be material, I ſpeak from the recollection of ſome weeks ago.

Manner in which the voice iſſues from the apparatus.

In the exhibition, the Inviſible Girl is ſuppoſed or pretended to be included in the ſphere, and the converſation is held by ſpeaking into any one of the trumpets, and the reply comes out of them; that is to ſay, it is moſt clearly heard by applying the ear to one of their mouths. The voice is low, as if conveyed from a diſtance through a tube, but it is very diſtinct. The lady converſes in ſeveral languages, ſings, deſcribes all that happens in the room, and diſplays a fund of lively wit and accompliſhment, that admirably qualify her to ſupport the character ſhe has undertaken.

Experiment of ſound conveyed to great diſtances by a thread.

I hope to meet my reader hereafter with a little more to ſay on the nature of this experiment. At preſent I can only offer the conjecture that the ſound is conveyed through the ſtrings, and to this I am led by another experiment of the celebrated Dr. Moyes. He ſhewed me, that if one end of a packthread, or even ſewing thread, be wrapped round the finger, and that finger be put into the ear (not forcibly) while the other ear is ſtopped, and at the ſame time an aſſiſtant wraps the other end of the ſtring round the handle of a tuning fork, and (the ſtring being then drawn rather tight) he cauſes the fork to ſound, too low to be heard by himſelf and the byſtanders, it will nevertheleſs be very audible to him who holds the other end of the ſtring. We tried it at an interval of fifty feet, and the Doctor has tried it at upwards of three hundred, without any ſenſible diminution of the effect. I wrapped the ſtring round my wriſt, and put my finger in my ear, and found the effect very nearly as ſtrong. If inſtead of the tuning fork one obſerver bites hold of the ſtring between his fore teeth, while the other applies it as before to his ear by means of his finger, the voice of the former may be tranſmitted; but it is more difficult to manage this experiment with ſucceſs than the other.

Converſation through a ſtring.

Aeroſtatic Experiments of GARNERIN.

Revival of aeroſtation.

A new generation has ſprung up ſince the epoch at which Air Balloons conſtituted the object of aſtoniſhment and ſpeculative reſearch about twenty years ago. They who remember that buſy period as if it were yeſterday, muſt feel a little ſurprized at the viciſſitudes of human affairs, when they find how many of thoſe who are now intirely engaged in ſociety, were then ſcarcely in exiſtence; and how few have ſeen a balloon. The ſpectacle exhibited by Mr. Garnerin, though much leſs impreſſive than the experiments of his countrymen to whom this invention is due, in all their novelty and originality have nevertheleſs been received with much intereſt. The readers of our Journal will recollect him as the firſt adventurer whoſe

Garnerin the firſt operator with a parachute.

intrepidity and ſkill * enabled him to deſcend through the atmoſphere by means of a Parachute; and thoſe who extend their reſearches to the new relations and conſequences of uncommon experiments, will probably conſider theſe daring attempts as the embryon efforts towards the art of flying; which ſo many eminent men have thought within the reach of human power. He has not, however, yet, in the uncertain climate of our iſland, been able to gratify us with the ſight of his deſcent in this manner. Three aerial voyages, perfectly reſembling thoſe of his predeceſſors, have conſtituted the whole of what he has yet been enabled to perform.

Three voyages in England.

The firſt with Capt. Sowden.

On the 28th of June he made his firſt aſcent from Ranelagh Gardens. After the promiſe of a fine day by the indications of the morning, the weather changed, and at five o'clock, when the whole was in readineſs, there was a ſtrong wind from the ſouth weſt, with occaſional ſhowers. At this honr Mr. Garnerin and Capt. Sowden aſcended, and proceeded over St. James's Park, the Thames, and the bridges of Weſtminſter and Blackfriars. After proceeding over the metropolis they threw out a quantity of ballaſt, and aſcended to an elevation which Mr. Garnerin eſtimates at upwards of 10,000 feet above the ſurface of the earth, or nearly two miles perpendicular. At the expiration of about half an hour the valve was opened, and the balloon deſcended through dark clouds which had intercepted their view of the earth, upon ſight of which they found themſelves ſpeedily advancing towards the ſea. On account of the tempeſtuous weather their landing was very dangerous and unpleaſant. The wind dragged them over fields and hedges, which tore their hands and cloaths, and when their anchor had engaged itſelf at laſt in a thicket near a houſe, the inhabitants were ſo far from aſſiſting them, that they even appeared diſpoſed to fire upon them. For want of this neceſſary help their cable broke, and they were dragged forward till the balloon was ſufficiently lacerated to begin to collapſe, and ſuffered them to jump out.

Aſcended two miles.

Dangerous landing.

Singular narrative of Captain Sowden.

Capt. Sowden publiſhed an account of this excurſion, in which he relates ſome ſingular obſervations, conſiderably differing from thoſe of former aeronauts. He affirms, that his ſight in looking down was ſo ſtrong, that he could eaſily diſtinguiſh

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* Philoſ. Journal 4to. I. 523.

guiſh the minuteſt objects on the earth. In this vaſt Map, he could not only trace the different roads and interſections, but even the ruts and furrows in the field. His hearing became likewiſe ſurpriſingly acute. The rattling of the carriages on the roads, the lowing of cattle, and the acclamations of the populace, were diſtinctly heard at the height of fifteen thouſand feet; though at the ſame time the travellers could ſcarcely hear themſelves ſpeak. He is perſuaded that a perſon with a ſtrong voice and a ſpeaking trumpet, might be perfectly underſtood from the earth at that elevation in the air. Inſtead of finding the climate colder as he aſcended, he obſerved the contrary.

Account of their landing near Colcheſter, after travelling at the rate of 80 miles per hour.

The place of their deſcent was on a common four miles beyond Colcheſter and ſixty miles from Ranelagh, and the time from their departure was three quarters of an hour; conſequently they travelled at the rate of eighty miles an hour. They were very hoſpitably received by Mr. Kingſbery, a freeholder of the county, who imagined their viſit to be on account of the parliamentary election, in which he had determined not to interfere. By his very friendly ſervices they were refreſhed and proceeded to Colcheſter, whence they returned to London on the following day.

Second voyage, July 5, 1802.

The ſecond voyage was made on the fifth of July, from Lord's cricket ground, near Paddington. The very unfavourable weather rendered his aſcent much leſs ſtriking than it might elſe have been. Thouſands of individuals filled the ſurrounding fields for ſome hours before the aſcent, amidſt rain nearly inceſſant, and a continual haze. The balloon roſe with Mr. Garnerin, accompanied by Mr. Locker, at ten minutes before five in the evening, and in four minutes afterwards it was loſt in the maſs of clouds. At the end of the fifth minute they began to deſcend, and landed at Chingford in Eſſex, after having remained in the air for one quarter of an hour. The moſt remarkable obſervation in Mr. Locker's account is, that neither himſelf nor Mr. Garnerin could diſtinguiſh ſounds above the elevation of 3 or 4,000 feet.

Pyrotechnic balloon, July 20.

On the 20th following a balloon was ſet up from Vauxhall gardens in the night, having an extenſive ſyſtem of fire works attached to it, to be diſcharged by means of a fuze, at a great height in the air previous to the exploſion of the balloon itſelf.

This exhibition, which does not require any general philoſophical explanation, was attended with the moſt complete ſucceſs, and had a very ſtriking effect.

Third voyage, with Mrs. Garnerin and Mr. Glasſurd.

The laſt voyage was made from Vauxhall Gardens by Mr. Garnerin, accompanied by Mrs. Garnerin and Mr. Glasſurd, on Tueſday, the 3d of Auguſt, at a quarter paſt ſeven in the evening. The day was calm and ſerene, and Mr. Garnerin made the offer of deſcending by the parachute, which he ſays in his letter was rejected, but not by whom. Circumſtances were highly favourable to the proſpect. The travellers paſſed ſlowly over the metropolis in bright ſun-ſhine, at an elevation which permitted them to be diſtinctly ſeen by a good perſpective magnifying about 30 times, and for the moſt part they were much more clearly ſeen. A cat attached to a parachute was let fall from an height of about 600 feet. It deſcended ſwiftly at firſt, till the parachute was fairly expanded, and then came down ſlowly and with ſafety. They landed near Hampſtead, about two miles north of the ſkirts of the town.

Promiſe of a diſſertation on this object.

Notwithſtanding the very ample diſcuſſion the ſubject of aeroſtation has had at the period of its invention, I apprehend that my readers will not be diſpleaſed to ſee an outline of what relates to it, with an inquiry into the probabilities of its becoming of any extended degree of utility; and this I purpoſe to do in our next.

Commercial College at Hull.

Objects of a propoſed commercial college.

Dr. J. Alderſon of Hull, has given out propoſals for eſtabliſhing a college in that town, for purpoſes highly intereſting to that commercial intercourſe which has in ſo many reſpects extended the powers and enjoyments of the civilized ſocieties of modern times, and is ſo intimately connected with the improvement of the intellectual and moral habits of man. He ſtates as the objects of the eſtabliſhment, *1ſt*, To obtain for the man of buſineſs preciſe information on the nature and value of every article of commerce, whether crude or manufactured; *2d*, To point out the country where ſuch articles are beſt to be procured; *3d*, To render him acquainted with the various proceſſes by which they are rendered marketable; and, *4th*, To inſtruct him in the languages of the different countries to which the objects of commercial intercourſe may lead him.

The

Plan.

The plan is to form a muſeum, and to erect a building for the accommodation of Profeſſors, and the reception and diſplay of ſpecimens of raw materials and manufactured articles. One Profeſſor to be engaged to teach the ſouthern languages, another the northern, and a third to have the care of the muſeum and the office of explaining whatever relates to the ſpecimens under his care. The eſtimate for the building and ſpecimens, to be raiſed by ſubſcription, is 2,500l.; and it is propoſed that the corporations ſhould endow the Profeſſors with 50l. per annum each.

Its great advantages.

The Doctor, who ſtands in the honourable ſituation of propoſer of an eſtabliſhment ſo manifeſtly uſeful, diſclaims the notion of this being a charitable inſtitution; but conſiders it as the private intereſt of every ſubſcriber. "As the father of a large family," he obſerves, "I certainly ſhould not think myſelf at liberty to ſubſcribe fifty pounds merely for the perſonal pleaſure that reſults from the contemplation of a charitable action; but in ſubſcribing fifty pounds to the above inſtitution, I conſider my intereſt and the welfare of my family. I am convinced it will add to the wealth and importance of the town; and in both of theſe I am deeply intereſted: but the greateſt intereſt I have, is in the facility it will give to the education of my children. In a commercial country and a ſea-port town, the acquiſition of knowledge that evidently leads to the improvement of trade, becomes an object of the firſt moment in the education of youth; and the mode now propoſed offers advantages which no private ſchool can poſſeſs. To bring together under the immediate obſervation of youth (while yet the memory is moſt retentive), perfect ſpecimens of the different articles of commerce, both of the raw material and of the manufactured, will enable him to bear in mind the preciſe value of any article he may be called upon to appreciate: To point out the country where every article is firſt procured or manufactured, will qualify him to go to the cheapeſt market, and by teaching him the language of ſuch country, he will be at all times capable of tranſacting his own concerns without the intervention of interpreters, often a very ſerious ſource of impoſition."

It will no doubt be of great advantage to the town of Hull, that this plan ſhould meet with ſupport, and to the community at large, that it ſhould be followed in other places.

A Syſtem of Chemiſtry in Four Volumes. By THOMAS THOMSON, *M. D. Lecturer on Chemiſtry in Edinburgh.*

Thomſon's chemiſtry.

This work of Dr. Thomſon (who is well known to the public by his excellent notes on an edition of Fourcroy's chemiſtry in three volumes, as well as by ſeveral important memoirs in our Journal, and other productions) is intended to exhibit a detail of the vaſt number of facts which conſtitute the ſcience of chemiſtry, blended with the hiſtory of their gradual developement and of the theories which have been founded on them, and accompanied with exact references to the original works in which the different diſcoveries have been regiſtered. His plan or order of arrangement will be beſt ſeen by enumerating the contents.

After a ſhort introduction, it is divided into parts, books, chapters, and ſections. Part firſt relates to the principles of chemiſtry. Simple ſubſtances, viz. oxigen; ſimple combuſtibles, viz. ſulphur, phoſphorus, carbon, hidrogen, azote: Metals, 22.—Light; caloric; general obſervations.—Compound ſubſtances. Primary: Alkalis; earths, 10; oxides; acids. Secondary: Glaſs, ſalts, hidro-ſulphurets, ſoaps.—Of affinity.—Examination of nature. Of the atmoſphere; of water; of minerals; of vegetables; of animals.

To enter into a detail of the numerous intereſting and highly valuable facts and ſtatements which abound under the heads above enumerated, would be impracticable in the ſhort limits of a notice. I muſt therefore content myſelf with obſerving, that it is a clear, comprehenſive, and accurate ſyſtem, in which the learner will be orderly led from one truth to another, and the man of information will with eaſe find the compendium and references he may want; that it is beautifully and cloſely printed on fine paper, with ſide notes; and upon the whole muſt prove a deſirable acquiſition to our ſtock of means for the advancement of chemical knowledge.

Death of THOMAS GARNET, *M. D. late Profeſſor of Chemiſtry and Natural Philoſophy at the Royal Inſtitution, &c. &c.*

Death of Dr. Garnet.

On the 28th of June laſt died of a typhous fever, caught in the gratuitous exerciſe of his profeſſion, Dr. Thomas Garnet, a man who, for his philoſophical knowledge, his aſſiduous exertions in diffuſing information by his lectures, and his amiable and unaſſuming diſpoſition in ſociety, will be long regretted by the world, and by his friends in particular. He was a native of Weſtmoreland, and after a regular education, ſtudied phyſic at Edinburgh, which he afterwards practiſed at Harrowgate. Subſequent to this period, he commenced a courſe of lectures on chemiſtry and experimental philoſophy at Liverpool, which he repeated at Mancheſter with ſuch ſucceſs, that when a vacancy offered in Anderſon's Inſtitution at Glaſgow, he became a candidate for the appointment, and obtained it. In this ſituation he acquired ſo high reputation, that he was applied to by the Managers of the Royal Inſtitution to be the firſt Lecturer in that eſtabliſhment, which unſolicited honour he accepted, and delivered a very extenſive and truly laborious courſe of lectures during the commencing ſeſſion of that corporate body, which were followed by the firſt ranks in audiences ſo crowded, as perhaps were never before witneſſed on a ſimilar occaſion.

Some account of his life.

Before he quitted Harrowgate he married a Miſs Cleveland, whoſe talents and accompliſhments were peculiarly adapted to his own. With this amiable friend and partner of his fortunes he paſſed a few ſhort years happily, and became the father of two daughters. But the birth of the latter infant was fatal to her mother. It was ſoon after this event, and during the early exertion of his fortitude under ſo great an affliction, that he became attached to the Royal Inſtitution. His conſtant attention to his numerous occupations, ſupported by the hope of ſecuring an independence for himſelf and the orphan repreſentatives of their regretted parent, afforded the moſt rational means of conſolation. But the energies of the mind cannot ſupport the phyſical ſtructure under accumulated labours, anxious reflections, and the ſolitary privation of that domeſtic ſociety which choice and habit had rendered dear to the human heart. His health became impaired, but his determination to proceed, continued in all its vigor. He took an houſe in Marlborough ſtreet, increaſed his ſtock of apparatus, completed a lecture room, and his claſſes became highly reſpectable under him as the lecturer on his own private account. At this period it was, when he had applied the whole of his means and the utmoſt ſtretch of his powers to the well grounded plan of eſtabliſhing himſelf in the uſeful and honourable profeſſion

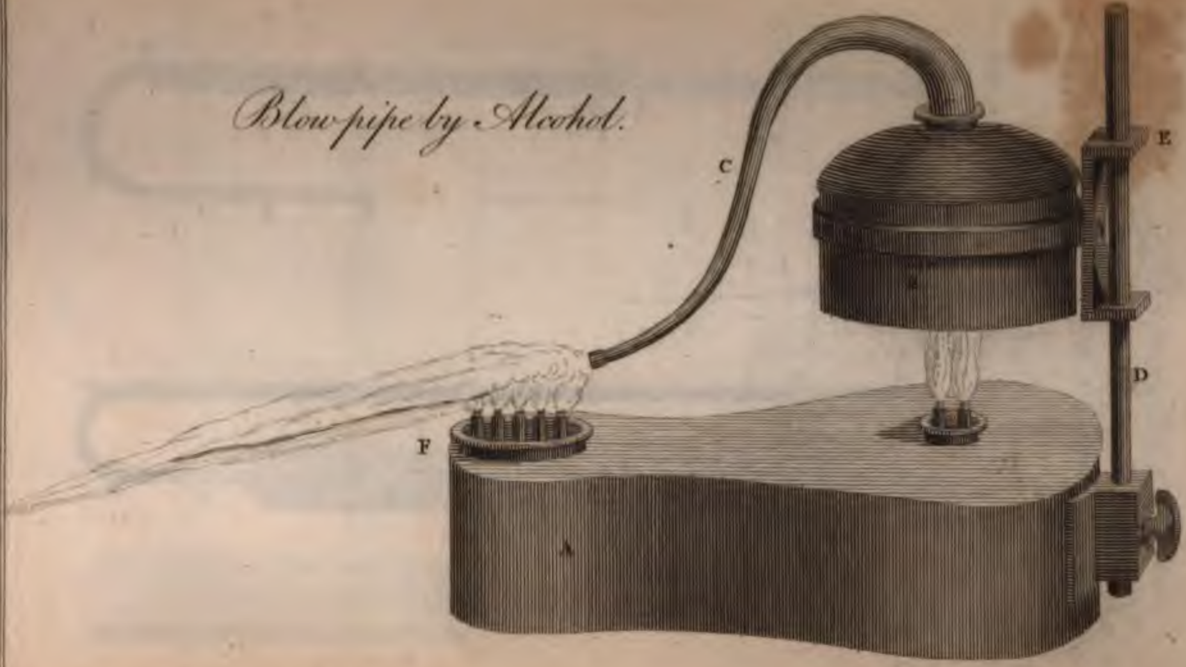
Some accounts of Dr. Garnet.

he had adopted; at this period it was that death terminated all his hopes, and left his infants too young to feel the poſſible extent of their misfortune!

I have ſaid the *poſſible*, and not the *probable* extent of their misfortune. He himſelf in the lateſt moments of his life muſt have known—nay he did know—that the public he had endeavoured to ſerve and to inſtruct, have ever been alive to caſes much leſs powerfully claimant on their feelings, their generoſity, and their juſtice. Theſe children cannot be forſaken by the numerous and affluent individuals who eſteemed his virtues and revered his talents. When it ſhall be known that his Lectures on Zoonomia are to be publiſhed by a ſubſcription deſtined to the purpoſe of affording them an humble, but independent ſupport, they will moſt aſſuredly find the loſs of their parent ſupplied in this reſpect, by that ready benevolence which is ſo peculiarly characteriſtic of our country. It is intended to addreſs the Public at the ſeaſon when the town fills; but in the mean time the Royal Inſtitution have permitted the work to be printed at their preſs, and have ordered a donation worthy of the ſpirit and liberality of their views. Several gentlemen have alſo ſubſcribed, and I am authorized to aſſure the friends of this laudable undertaking, that ſubſcriptions are received at the houſe of the Royal Inſtitution. I ſhall myſelf be very happy to afford any farther information to enquirers.

*** Mr. Notlem will have the goodneſs to point out the Volume and Page of the paſſage for which he wiſhes to have an explanation.

Blow-pipe by Alcohol.



Mutlow Sc. Russell Ct.

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Barometers.

by the Rev. James Wilson, A.M.

Fig. 1.

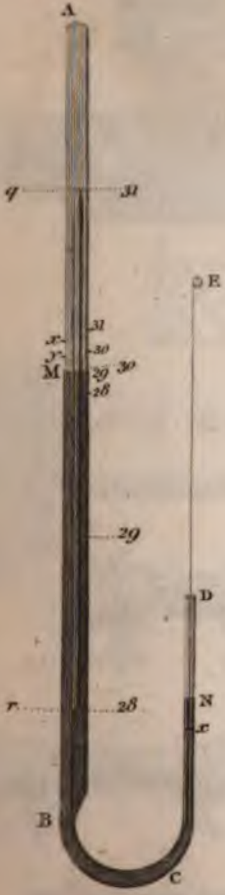
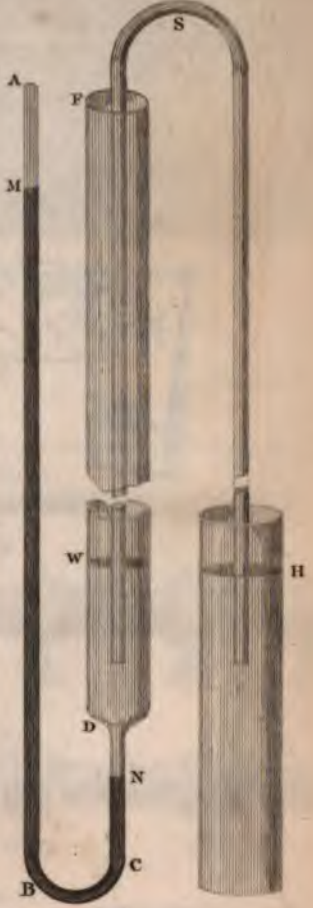


Fig. 2.



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Mr. Besant's Water Wheel.

Fig. 1.

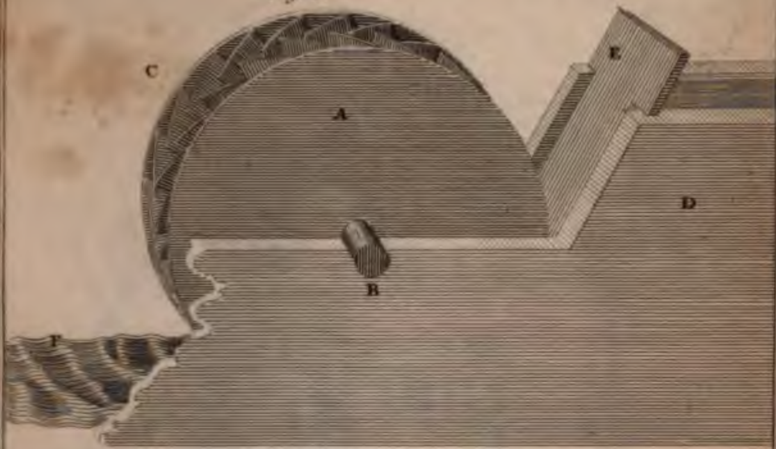
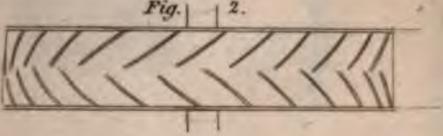


Fig. 2.



Mr. R. Phillips's Tubes for driving Bolts into Ships

Fig. 3.

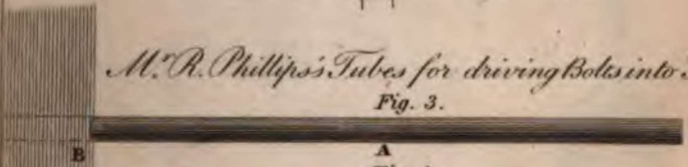


Fig. 4.

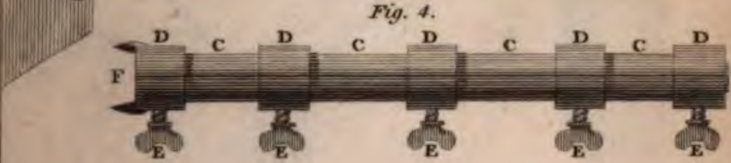


Fig. 5.

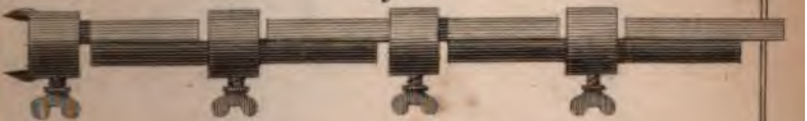
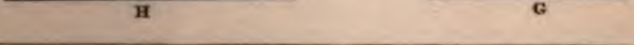


Fig. 6.



Mutlow Sc. Ruſsell Ct.

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Mr. Besant's Water Wheel.

Fig. 1.

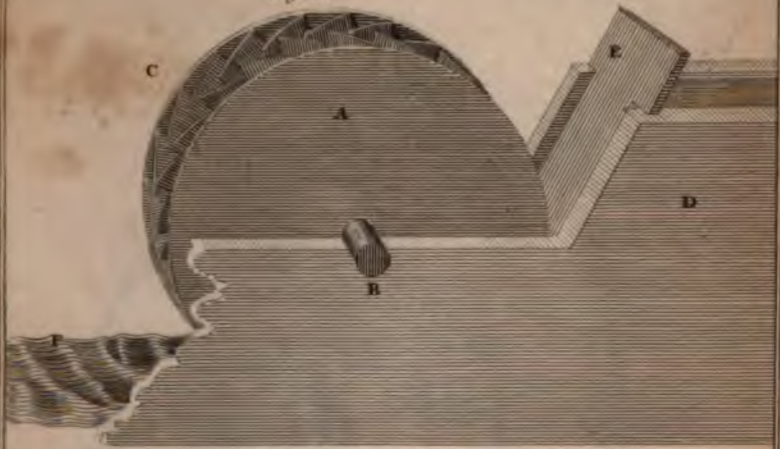
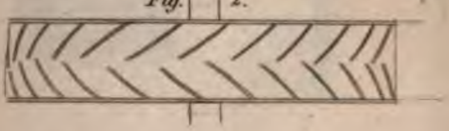


Fig. 2.



Mr. R. Phillips's Tubes for driving Bolts into Ships

Fig. 3.

Fig. 4.

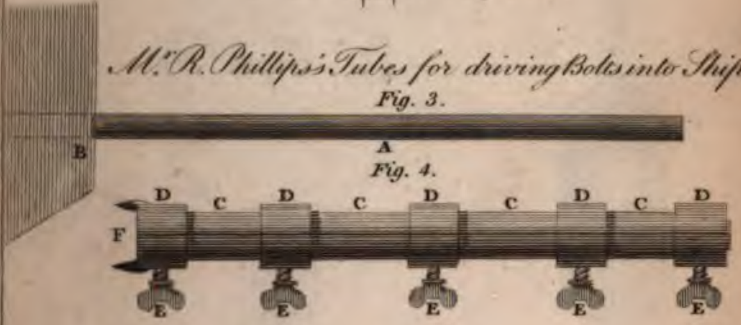


Fig. 5.

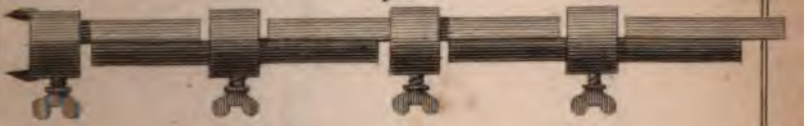


Fig. 6.

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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1802.

ARTICLE I.

Obſervations upon ſome remarkable Wells near the Sea Coaſt at Brighthelmſtone, and other Places contiguous. By the Rev. W. Pearson, *P. R. I. From the Author.*

The wells near Brighthelmſtone are ſaid to be empty at high water, and full at low water,

at Rottingdean.

IN Fiſher's "Deſcription of Brighthelmſtone, and the adjacent country," (p. 38, fourth edition) it is ſaid that "*water* is procured at Brighton from wells of conſiderable depth, and being filtered through chalk, of which theſe parts are one immenſe rock, it is of courſe perfectly cleared of every ſpecies of foulneſs. The following phenomenon is obſervable in ſeveral of theſe wells; *at the time of high water they are empty, and at low water they are full;*" likewiſe under the article *Rottingdean* of the ſame pamphlet we read, "On the road to Newhaven, at the diſtance of near four miles from Brighthelmſtone, lies the pleaſant and delightful village of Rottingdean. This place is remarkable for its wells, which *are nearly empty at high water, but which riſe as the tide declines.* Theſe aſſertions appeared on peruſal ſo ſingular, that I formed a determination of availing myſelf of the opportunity which a temporary viſit to this faſhionable bathing place gave me in the month of July laſt, of examining how far the ſtatements in queſtion are *accurate;* accordingly I obtained a plumb line, and meaſured the depths of the *ſurface* and *bottom* reſpectively of ſeveral wells in different weeks, and at different hours of the tide; the reſult of which meaſurements turned out to be a convincing proof that this traditionary report, though partially founded in fact, has been greatly exaggerated; but whether or not for the purpoſe of entertaining the viſitors of this place, it would be preſumptuous to determine. At the time, however of my viſit, it was not my intention to lay before the philoſophical reader any obſervations which were made, but merely to regiſter them by way of private amuſement; which intention would have been adhered to, had I not met with a paragraph in the General Evening Poſt of Tueſday, Aug. 24, 1802, of which the annexed is an exact copy.—"Rottingdean, four miles from Brighton, on the road to Newhaven, is remarkable for the ſingular variation of its wells, which *riſe as the tide declines, and are nearly empty at high water!* The reſearches of philoſophy have not as yet been able to affix the cauſe of this apparent contradiction in the courſe of nature." This attempt to excite a more general aſtoniſhment than was excited by the Brighthelmſtone guide alone, and conſequently to call the attention of the philoſopher to inveſtigate the ſubject before us, has induced me to recur to my notes, and to tranſmit for your Journal the deductions I had made from them, in order to ſhow that there is no *apparent contradiction* in the *phenomenon*, as it has been called, of the Brighton Wells.

This report is not accurate,

but was lately repeated in a reſpectable newſpaper.

The South Downs, which conſtitute a ridge of high ground to the north of Brighton, the ſummit of which is parallel to the ſea coaſt, have their *gradual* ſlope inclining towards the ſea or ſouth ſide, and the declivity, which approaches more to a perpendicular line, facing the level country on the north; and the *dip* of the ſtratum of chalk, and conſequently of its impermeable bed, is in a direction from the ſummit to the ſea, or from north to ſouth; the rain therefore which falls upon theſe Downs may be expected to run down the bed of the rocks until it arrives at their lower extremity, where, if it meet with any oppoſing clay, or other impermeable ſubſtance, it will collect into a body of water, and, as it collects, riſe through the crevices of the rock and ſuperincumbent permeable earth, till it ſhews itſelf in the form of ſprings. This reaſoning, if my recollection be perfect, is agreeable to the theory of Mr. Pilkington, whoſe ſucceſs in draining and tapping ſprings

The South Downs lie on a bed of chalk ſloping towards Brighton;

upon which the rains are conveyed;

and form ſprings at its lower extremity;

ſprings is now pretty generally known; and in conformity to it I was led to ſearch in the firſt place for the lower extremity of the rock, which I found was *upon the ſhore,* near the ſeparation between the rounded ſtones and ſand, at from thirty to forty yards by eſtimation below the bank. Here, as I had previouſly ſuppoſed, I found the freſh water continue to ſpring or guſh out conſtantly in a line extending parallel to the bank, both to the eaſt and weſt of Brighton as far as I examined. In this line then are ſituated the *natural ſprings* of the place, which, if the ſea were remote, would be not only perennial, but probably would emit an equal * quantity of water at all times; but in every tide the ſea covers this line of natural ſprings, from three to ſix feet we will ſuppoſe, according to the ſtate of the moon, wind, &c. ſo that the freſh water is prevented from making its eſcape by the heavier ſuperincumbent ſalt water of the ſea, the conſequence of which is, that, from the time the ſalt water covers this line, the freſh water begins to accumulate in the bed of the rock and inclined rock itſelf, as high at leaſt as the ſurface of the tide. During the time therefore that the tide continues above this line, the freſh water is pent up in the rock at a diſtance ſufficiently remote from the ſea, to exhibit a perceptible riſe in the artificial wells ſouthward of the bank, which are dug down into the bottom of the rock. That this ſhould be the caſe to a certain extent it was natural to expect, but *what period of time ſhould elapſe* between the inſtant of the tide's arriving at the line of the natural ſprings, and the inſtant of a perceptible riſe commencing in the artificial wells, at ſome diſtance from that line, can only be aſcertained by actual obſervation, for both the diſtance of the wells, and number as well as capacity of the ſmall natural ducts that convey the water muſt be taken into the conſideration, if an attempt were made to inſtitute a calculation, and I confeſs my memoranda are rather deficient in this reſpect; my attention having been directed more particularly to aſcertain the ſtates of the tide when the wells were at the two extremes of riſing and falling, which times are erroneouſly aſſerted to be about ſix hours from being contemporary with the

which make their way into the ſea at the diſtance of thirty or forty yards below the high-water bank.

Theſe ſprings are covered by the ſea every tide;

and conſequently the upper water is prevented from coming down;

and the wells become fuller;

not at *high water* but *later.*

Obſervations as to the facts.

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* The reaſon for this ſuppoſition is, that the quantity of inclined ſurface is very conſiderable, and is alſo contiguous to the ſea, where ſhowers are frequent.---W. P.

influx and efflux of the tide. The two wells, which were moſt convenient for examination, were, one immediately behind Fiſher's library, and another in a little office at a ſmall public houſe called the Dolphin, near the foot of Eaſt-ſtreet, which the fiſhermen uſe for procuring water for tanning their nets; which two were nearly equally diſtant from the bank, and ſubject to the ſame alternations of riſing and falling.

Deductions.

The deductions which were made from a variety of meaſurements were as follow:

The wells are fulleſt during the ebb.

1. The wells are moſt filled with water when the tide is retiring from the line of the natural ſprings; evidently becauſe the accumulation of freſh water continues as long as the ſalt water is above the ſaid line.

The ſprings are covered four hours by the ſea;

2. The whole continuance of the tide above the line of the ſprings, or time of filling, appears on an average about four hours.

and uncovered ſix hours.

3. The whole time of emptying ſeems to be about ſix hours.

A ſhallow well becomes quite empty; a deeper not ſo.

4. The well at the Dolphin is about three hours quite empty in every tide, but the other, which is deeper, is never quite empty.

Particulars, well known.

5. At two hours after high water on the 16th of July, viz. one day and a half after full moon, the water in the well at the Dolphin was two yards deep.

6. The fiſhermen who uſed the well at the Dolphin, informed me that they *knew very well* that the water in the wells roſe to the greateſt height at *two hours* after high water, and were at the loweſt *two hours* after low water, which information accords with my obſervations.

The water freſh.

7. The water always taſted freſh and pleaſant.

A remote well ſcarcely varies.

8. A well about 170 yards more remote from the ſea than that of the Dolphin, in a direction towards the market, never varied more than three inches and a half in depth at the different times of obſervation.

Why the deeper well holds its water.

9. The well near Fiſher's is eight yards and 30 inches deep, which depth is probably below the line of the natural ſprings, and therefore it contains water at all times; but the other is not ſo deep, and therefore loſes all its water every tide.

The effect has thus been correctly ſtated and accounted for.

From theſe deductions it will, I preſume, appear evident not only that the accounts which have been circulated reſpecting the wells at Brighton, Rottingdean, and other villages contiguous to the Suſſex coaſt, are erroneous, but alſo that a ſatisfactory reaſon may be given for their reciprocation, which, though not contemporary with the ebbing and flowing of the tide, is yet ſo far dependent upon it, as to follow it at an interval of two hours, but in ſuch a way that the *immediate influence* of the tide upon it only laſts about four hours out of the twelve, notwithſtanding the effect produced in that time requires nearly ſix more to put the wells into their original ſtate of vacuity or reſt.

It reſembles a river tide, produced on the freſh water by the ſea: like that it is later; and has its ebb longer than the flood. If the ſtream flowed down a declivity higher than the higheſt tide, the ſtream would always ebb.

Laſtly, the ſame reaſoning which accounts for the alternate riſing and falling of the water in the artificial wells, makes it more than probable, that if the line of the natural ſprings had been below the low-water mark, the artificial wells would have been not only required to be deeper, but would have been invariable in quantity; alſo, that if the ſaid line had been above high-water mark, it would have been in vain to have dug for water into the rock above, whilſt there continued a natural iſſue below: and ſhould it be aſked, why the water that has an immediate communication occaſionally with the ſea yet continues *freſh*, the ſame anſwer may be given that will apply in the caſe of freſh water being driven back to Chelſea and Putney at every ſucceſſive tide that enters the mouth of the Thames.

Parſon's Green, Aug. 29, 1802.

II.

On the Theory of Galvaniſm. By JOHN BOSTOCK, *M. D.*

From the Author.

THE following Paper contains ſome remarks upon the electrical pile of Volta, principally made with a view to illuſtrate and confirm the theory of its action, which I propoſed in my former communication.

Galvaniſm and electricity are the ſame.

1. With reſpect to the nature of the agent producing the phenomena of galvaniſm, there can ſcarcely remain any reaſonable doubt of its perfect identity with the electric fluid. The apparent diſſimilarity in the effects of an electrical machine and a galvanic pile has been ſufficiently accounted for, without ſuppoſing any thing eſſentially different in their nature, and we ſeem enabled by proper management to produce from either

But electricity is *produced* in the pile.

The pile is conſidered as a *generating*, and the electric machine as a *collecting* apparatus.

either apparatus at pleaſure every effect of galvaniſm ſtrictly ſo called, or of electricity. There appears however to be a great difference in their mode of action; in the pile the fluid is actually generated, whereas in the machine it is merely transferred from one body, where it previouſly exiſted in a diſengaged ſtate, to another body. In ſupport of this poſition it may be alledged, that the energy of the pile is found to be materially increaſed by being inſulated, whereas in the common machine it is impoſſible to produce any conſiderable degree of excitement *, unleſs there be a communication formed by a conductor between the rubber and the ſurface of the earth. Dr. Woollaſton, who has thrown ſo much light upon this ſubject, ſeems to have carried too far the analogy between the methods of exciting the electric fluid, when he conſiders the oxidation of the amalgam on the cuſhion, as more than a ſecondary cauſe in the operation of the electrical machine. By conſidering the pile as a *generating*, and the other as only a *collecting* apparatus, we may aſſign a probable cauſe for the vivid ſparks, the bright flaſhes, and the more conſiderable marks of excitement which are exhibited by the common machine; in this latter caſe a quantity of electric fluid derived from the neighbouring bodies, is as it were forcibly detained in the apparatus, and is ready to eſcape to every ſubſtance for which it poſſeſſes any degree of affinity; whereas in the pile, though there is a rapid and conſtant production of electricity, yet being derived from a change which takes place in its own compoſition, by which different parts of it appear to acquire different capacities for the electric fluid, it ſeems little diſpoſed to enter into any combination with the ſurrounding bodies, and is only brought into action by the approximation of the different parts of the apparatus itſelf.

The effects of the pile has been aſcribed to its great quantity of electricity at a low intenſity;

It was juſtly remarked by Mr. Nicholſon, that the peculiar effects of the pile may be aſcribed to the electric fluid exiſting in it in a large quantity, but at a low degree of intenſity. This conſideration eaſily explains all the ſingular properties of the apparatus, but it ſtill remains to be aſcertained by what means this difference of intenſity is produced. By different degrees of electrical intenſity it is generally underſtood, that the ſame quantity of uncombined * electric matter may be diffuſed through a greater or leſs ſpace, and in conſequence of this, as it exiſts in a ſtate of concentration or dilution, it will exhibit effects more or leſs violent. In the galvanic pile it might have been ſuppoſed from the ſmall bulk of the apparatus, that a large quantity of electric matter could not have exiſted in it except in a concentrated ſtate, but we find that the contrary is invariably the caſe; nor has any method been hitherto contrived by which the electricity of the pile can be made to exhibit the ſame marks of intenſity with that of the common machine. Future experiments will probably enable us to determine with more preciſion in what the difference between a greater or leſs intenſity conſiſts; poſſibly the electric fluid, as is the caſe with many other bodies, may differ in its nature according to the method employed for extricating it from the ſubſtances with which it was previouſly united.

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* This is an overſight. In fact, the ground ſerves only to form a communication between the receiving conductor and the cuſhion, and the excitation is fully as ſtrong when the current is made to fly from the plus to the minus conductor, when both are inſulated.---N.

but the nature of the differences of intenſity have not been explained.

2. That the extrication of the electric fluid in the pile of Volta depends upon the rapidity with which the ſurface of the metal is oxidated, after the experiments which have been performed, and particularly thoſe of Mr. Davy, ſeems to require no farther proof. It is however not only neceſſary that the fluid ſhould be extricated, but that when diſengaged it be concentrated and carried forward in a uniform direction. This conſideration clearly explains the reaſon why in the conſtruction of a pile there muſt either be two metals employed, which are differently acted upon by the interpoſed fluid, or that the different ſurfaces of the ſame metal be ſubjected to the action of different fluids. Dr. Van Marum conceives it impoſſible that the extrication of the electric fluid can depend upon the oxidation of the metals, becauſe he found that the pile acted more powerfully when he uſed the muriate of ammoniac, than when a diluted acid was employed; but the objection of the learned experimenter is ſufficiently obviated by the conſideration ſuggeſted above. He farther alledges in ſupport of his opinion, that a pile with potaſh interpoſed between the plates acted very powerfully, though no oxidation was produced. I have repeated this experiment, and have on the contrary uniformly

The electricity of the pile depends on the rapidity of oxidation, &c.

Dr. Van Marum denies this.

particularly becauſe potaſh afforded galvaniſm without oxidation;

but the contrary proved to be the fact.

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* Or uncompenſated by its oppoſite.---N.

formly found the ſurface of the zinc covered with a white cruſt; in this caſe the water employed in diſſolving the potaſh, oxidates the zinc in conſequence of the attraction which ſubſiſts between the oxide of zinc and the fixed alkali.

Probably the diſengagement of electricity is always attended with oxidation in a metal.

We conclude it to be a general fact, that whenever a metal is oxidated the electric fluid is diſengaged, and it is probable that the reverſe of this propoſition will be found to be true, viz. that whenever electricity is diſengaged from a metal it becomes oxidated. It will appear therefore, that the experiments which were made with ſo much accuracy by Sig. Volta, upon the different electricities produced in metals by mutual contact, though certainly inadequate to form a complete theory of the action of the pile, are yet not to be diſregarded in our attempts to increaſe the power of the apparatus.

The true theory ſuppoſed to be that of Volta combined with that of oxidation.]

In order that its conſtruction be adapted for exhibiting the greateſt energy, it will be neceſſary both that the metals employed be ſuch as to produce a current of electricity when brought into contact, and alſo that the interpoſed fluid ſhould rapidly oxidate one of the metals without exerciſing any action upon the other. The combination of zinc and ſilver anſwers the firſt of theſe conditions, and accordingly affords the beſt materials for the formation of the pile. With reſpect to the interpoſed ſolution, the muriates, and particularly the muriate of ammoniac, have for the moſt part been found the moſt efficacious, probably in conſequence of the attraction which ſubſiſts between zinc and the muriatic acid. The muriate of ammoniac is known however to act readily upon copper as well as upon zinc, and yet we find that this ſalt in a ſtate of ſolution powerfully excites the energy of a pile compoſed of zinc and copper, a fact which ſeems to militate againſt our hypotheſis. With a view to illuſtrate this point the following experiments were performed:

Exper. I. Zinc expoſed to ſaturated ſolution of muriate of ammonia cauſed emiſſion of hidrogen, abſorbed ſome acid, and became white.
Exp. 2. Copper in like circumſtances was oxided and abſorbed acid, &c.

1ſt. A piece of zinc was expoſed to the action of a ſaturated ſolution of the muriate of ammoniac, a ſmall quantity of hidrogen gas was diſengaged, the fluid acquired an exceſs of alkali, and after ſome time the metal became covered with a white cruſt. 2. A clean plate of copper was placed horizontally in a ſolution of the muriate of ammoniac; after a few hours the upper ſurface of the metal was covered with a bright green cruſt, while the under ſurface became beautifully ſtudded with ſmall, tranſparent, and nearly colourleſs cryſtals. Theſe cryſtals after expoſure to the air aſſumed a green hue, ſimilar to that of the cruſt on the other ſide of the copper. This green cruſt was ſcarcely acted upon by water, but was rapidly diſſolved by ammoniac, and the ſolution became of a deep blue colour. It ſeemed therefore to be a muriated oxide of copper. The cryſtals were only in part ſoluble in water, the inſoluble part became browniſh; it was quickly diſſolved by ammoniac, and a deep blue was produced. The water in which the cryſtals had been digeſted was without colour or ſmell, but upon the addition of a few drops of cauſtic potaſh, it exhaled a ſtrong odour of ammoniac. Theſe cryſtals therefore appeared to conſiſt of muriate of ammoniac united to a quantity of the oxide, or muriate of copper. The ſides of the glaſs were lined with a coating of the green oxide, and as the ammoniac continued to evaporate, the quantity of this oxide was increaſed. The fluid in which the copper had been digeſted exhibited alkaline properties, and was of a deep blue colour; it contained the ammoniated oxide of copper. 3. A piece of zinc and a piece of copper ſimilar to thoſe employed in the former experiments, were placed in a ſolution of the muriate of ammoniac in contact with each other. The zinc was oxidated as in the former caſe, but the fluid remained colourleſs, and the copper acquired no green cruſt; though after being removed from the liquor and expoſed for ſome time to the atmoſphere, it gradually acquired a thin covering of oxide. The liquor appeared to conſiſt of muriate of ammoniac with a ſmall exceſs of alkali. In this experiment we may conjecture, that the zinc poſſeſſing a ſtronger attraction for oxigen than the copper, abſtracts it from the water, and the hidrogen which is then diſengaged either prevents the oxidation of the copper from taking place, or reduces the oxide as quickly as it is formed. 4. When the zinc and copper were placed in ſeparate glaſſes of the ſolution of the muriate of ammoniac, and the metals were connected by a ſilver wire, the zinc became oxidated as uſual: the copper was in appearance ſcarcely acted upon, but the fluid acquired a blue tinge, which proves that the ammoniated oxide of copper is formed, and conſequently that the muriate of ammoniac and water are decompoſed, and the copper oxidated, though theſe effects take place to a much leſs extent than in the ſecond experiment. The general reſults are favourable to our hypotheſis, as we

learn

Exper. 3. Zinc and copper in contact in a ſimilar ſolution; the zinc only was acted on.

Exper. 4. Zinc and copper expoſed as before, but in ſeparate veſſels, and communicating by a ſilver wire, the copper was little affected.

Hence a pile of zinc and copper may be conſidered as if one of the metals

nly were oxid-
ble by mur.
cid.

learn from them that the action of the muriate of ammoniac upon copper is in a great meaſure ſuſpended when the copper is in contact with zinc.

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t the ends of
he pile.

3. Before we can arrive at a perfect theory of the galvanic apparatus, it is neceſſary to aſcertain what is the eſſential difference between the condition of the electric fluid, as it exiſts in the two ends of the pile. It is eſtabliſhed decidedly by the experiments of Van Marum, that the fluid exiſts in the poſitive ſtate at what is called the zinc end, and in the negative at the oppoſite extremity. When the materials are arranged in the order of ſilver, zinc, and card, it is evident that the end from which the electric fluid is diſcharged in the poſitive ſtate, is the one contiguous to the oxidating ſurface of the zinc. It is not altogether determined in what conſiſts the difference between the poſitive and negative electricities, but it is generally ſuppoſed to depend upon the fluid being contained in bodies in a greater or leſs quantity than their natural proportion. Let us examine how far this takes place in the pile of Volta. We may conſider each pair of metallic plates with the interpoſed fluid as a complete apparatus for evolving and receiving the electric matter; and to whatever extent the pile is increaſed, we ſhall have merely a repetition of the ſame action. Let Z, fig. 3, plate 8, repreſent the plate of zinc, F the interpoſed fluid, and S the plate of ſilver. At the ſurface of the zinc, *c d*, the electric matter is diſengaged in conſequence of the oxidation of the metal; this diſengaged electricity is carried acroſs the fluid, and enters the ſurface of the ſilver *e f*, and being then diffuſed through the body of this plate, the whole of the ſilver acquires the poſitive ſtate, and conſequently *i* becomes the poſitive and *k* the negative end of this little apparatus. Does the plate of zinc in this caſe become abſolutely or only relatively negative? It might be ſuppoſed that no other part of the zinc ſhould have its electricity charged except that which is oxidated; yet from the experiments of Van Marum and others we are led to conclude, that the zinc becomes abſolutely negative through its whole ſubſtance. Dr. Van Marum informs us, that when the wire of a Leyden phial was applied to the negative end of his large pile, the inſide of the jar became charged with negative electricity; and as there does not appear to have been any connection between the external coating and the other end

Direction of the current.

Limit of electrization.

of the pile, we muſt conclude that the zinc plate becomes abſolutely negative through its whole ſubſtance. If the zinc became negative only in relation to the quantity of electricity accumulated in the ſilver, it would follow that the ſame effects ſhould be produced whether the ſilver was connected with the negative end of the pile, or with any other metallic ſubſtance of equal bulk; but this is directly contrary to fact. Beſides a variety in the quantity or quality of the electricity, another material difference between the two ends of the pile is the direction of the current. While the electric matter is diſengaged from the oxidating ſurface, it ſeems impoſſible to imagine that it can be received again by the ſame ſurface from which it has been diſcharged, and which is continually diſcharging freſh quantities of the fluid. The electricity is therefore neceſſarily diffuſed through the interpoſed liquid, and from this is taken up by the oppoſite metallic ſurface. Upon the uniform direction of its current it is that the action of the apparatus eſſentially depends; and accordingly we find that its effects are ſuſpended when it is altogether ſurrounded with water, or even when the external edges of the plates are moiſtened. It is a very curious circumſtance in the operation of this apparatus, that it does not appear capable of having its power augmented by permitting the electricity to become accumulated in it. The oxidating ſurfaces of the zinc are continually diſcharging a large quantity of electricity, all of which is carried forward to the poſitive extremity, and does not appear to eſcape from it; yet after the two ends of the pile have been brought to an equilibrium by means of an uniting conductor, the energy and intenſity of the apparatus are inſtantaneouſly reſtored to as great a degree as before the diſcharge. Upon the whole it may be inferred, that though the two extremities of the pile differ from each other in the abſolute quantity of electricity which they contain, yet there is reaſon to ſuppoſe that ſome farther alteration is effected, which at preſent we are unable to explain.

Effects at the ends of the wires.

4. With reſpect to the operation which takes place at the ends of the wires in the interrupted circuit, whatever idea we may adopt of the comparative ſtates of the electric fluid in the two extremities of the pile, we can ſcarcely doubt that it proceeds from the poſitive to the negative end, or from the wire which emits oxigen to that which emits hidrogen. The oxigen which is diſengaged from the poſitive or diſcharging wire, muſt neceſſarily

ſarily either proceed from the wire itſelf, or be diſengaged from the fluid in which the wire terminates. If it proceed from the wire itſelf, we may conclude that in whatever fluid the wire is plunged, the oxigen will ſtill be emitted, except the fluid ſhould itſelf poſſeſs an attraction for oxigen. Upon the other ſuppoſition, however, that the oxigen proceeds from the decompoſition of the fluid, in conſequence of the electricity uniting itſelf to hidrogen in its paſſage between the wires, we may expect to be able to ſuſpend the production of oxigen, by preſenting to the wire a liquid which contains hidrogen united to ſome other element. The liquid cauſtic ammoniac promiſes to anſwer all the requiſite purpoſes, and we ſhall find that the experiments made upon it by Mr. Cruickſhank and Mr. Davy, though not preciſely ſimilar in their reſults, afford concluſions not unfavourable to our hypotheſis. Mr. Cruickſhank employed wires of platina, and when theſe were connected with the pile, and permitted to terminate in ammoniac, he found that the gaſes evolved were azote and hidrogen, with ſo ſmall a mixture of oxigen as to render it probable that this latter ſubſtance proceeded rather from ſome accidental circumſtance, than from the eſſential nature of the operation. Mr. Davy uſed gold wires which terminated in two glaſſes of ammoniac, connected to each other by means of muſcular fibres. He found that the poſitive wire evolved gas only in ſmall quantity, and that it was a mixture of three parts of oxigen and two of azote, while the other wire gave out hidrogen in conſiderable quantity; the poſitive wire was viſibly corroded. I was for ſome time at a loſs to reconcile the reſults of theſe able experimenters, but upon reflection I was led to conclude that the variation might be produced in part from the nature of the wires which they uſed, and partly from a difference in the energy of their reſpective piles. In order that the experiment ſhould be unexceptionable, it is neceſſary that a metallic conductor be employed which poſſeſſes little attraction for any of the elements compoſing the fluid in which it terminates; as by this precaution any change of compoſition which takes place muſt depend altogether upon the affinities of the electric fluid. Gold is on this account leſs proper than platina. It may be conjectured that in Mr. Davy's experiment with gold wires, the prediſpoſing affinity of the oxide of this metal for an oxigenated nitric acid, would cauſe the decompoſition of a portion both of the

Immerſed in ammonia.

Azote and hidrogen evolved by platina wires in ammonia.

Gold wires;

corroſion.

the water and the ammoniac, and that the oxigen and azote thus liberated, would unite together in ſuch proportion as to generate a ſolvent for the metal. We ſuppoſe the gas which is diſengaged from the poſitive wire to be produced by the decompoſition of the fluid in which it terminates, in conſequence of the ſtrong attraction which ſubſiſts between the electric matter and hidrogen. It may reaſonably be inferred that there is a point of reciprocal ſaturation between theſe ſubſtances, or that a given quantity of electricity cannot paſs between the wires unleſs it be united to a definite quantity of hidrogen. If the electric matter in the poſitive wire exiſt there only in a ſmall quantity, it acquires for itſelf a ſufficient proportion of hidrogen by decompoſing the ammoniac alone; but when the apparatus is large and operates with energy, the ammoniac is inadequate to afford the neceſſary ſupply of hidrogen, and the water becomes alſo decompoſed. I endeavoured to ſubmit the truth of this conjecture to the teſt of experiment, for which purpoſe gold wires were employed terminating in ammoniac, and connected in different inſtances with a greater or leſs number of metallic plates. The reſults were upon the whole favourable, but on account of the minute quantities of gas procured, I was unable to aſcertain its nature with as much accuracy as was deſirable. When the proceſs went on rapidly the water was decompoſed as well as the ammoniac, and if it was continued for a great length of time, the ammoniac attracted carbonic acid gas from the atmoſphere, and thus produced a ſource of error in the reſults. Several other metallic bodies which were interpoſed between the ends of the pile, were all of them more or leſs corroded in conſequence of their ſtrong attraction for oxigen. When copper and tin were uſed, the oxides were diſſolved by the ammoniac; in the former caſe this was rendered viſible by the blue colour which the liquor aſſumed, and in the latter by the precipitation of the oxide of tin effected by the addition of ſulphuric acid. The ſlips of tin foil which were employed had their extremities changed to a brown colour, particularly that which was connected with the negative end of the apparatus.

The ſolution of ammonia has its alkali decompoſed by a weaker power, and alſo water by a ſtronger.

Experiment.

The minus wire of the pile.

After theſe obſervations upon the action of the poſitive wire, there remains little to be added on the ſubject of the oppoſite one; it merely receives the electric fluid after its paſſage through the water, and tranſmits it to the negative end of the pile. If the wire terminate in pure water, the hidrogen is diſengaged in the form of gas; but if a metallic ſalt be diſſolved in the water, the hidrogen is attracted by the oxide, and this laſt reduced to the ſtate of a metal. The phenomena exhibited in this proceſs are often peculiarly beautiful, and the extreme delicacy which the apparatus poſſeſſes in diſcovering even the minuteſt metallic impregnations, ſuggeſts its probable utility in the analytical operations of chemiſtry.

How far galvaniſm may modify our electric theories.

5. A very intereſting ſubject of inquiry here preſents itſelf, viz. in what degree will our theoretical notions of the action and properties of the electric fluid be influenced by the galvanic diſcoveries? The theory of Dr. Franklin, as well as the elegant one propoſed by M. AEpinus, ſeems to be adapted only to thoſe caſes in which the electric fluid previouſly exiſting in bodies, was altered either with reſpect to its diſpoſition or quantity. But we are now in poſſeſſion of an apparatus by which it is actually generated, a circumſtance with which we were until lately unacquainted, and conſequently our hypotheſis, being a general deduction from facts, will at leaſt require to be extended, perhaps in ſome inſtances new-modelled.

Chemical theory is alſo much benefited by theſe diſcoveries.

The pile of Volta affords many illuſtrations of the modern chemical doctrines, which appear the more ſtriking as effected by experiments made with a different object in view, and produced by an agent which cannot be ſuppoſed capable of affecting the reſults. The grand queſtion reſpecting the decompoſition of water, if it ſtood in need of farther ſupport, derives powerful confirmation from the experiments firſt performed by Mr. Nicholſon and Mr. Carliſle, and ſince repeated with various modifications. The experiments made with the galvanic apparatus alſo confirm our ideas reſpecting the compoſition of ammoniac and nitric acid, and conſiderably add to our knowledge of the action which takes place between acids and metals. They beautifully exhibit the powerful effects produced by oxigen and hidrogen when in a condenſed or uncombined ſtate, and promiſe to afford the chemiſt a valuable opportunity of employing in his different operations theſe bodies in a ſtate of abſolute purity. Every one intereſted in the progreſs of philoſophical knowledge, will regard with admiration the diſcovery of an apparatus, which has enabled him to develope ſome of the moſt hidden operations of nature, and will conſider the name of Volta as worthy to be claſſed with thoſe of Lavoiſier, Prieſtley, Cavendiſh, and the other illuſtrious founders of the pneumatic chemiſtry.

III.

A Summary of the moſt uſeful Parts of Hydraulics, chiefly extracted and abridged from Eytelwein's Handbuch der Mechanik und der Hydraulik. Berlin, 1801. *By* THOMAS YOUNG, *M. D. F. R. S.*

(Concluded from Page 35.)

Motion of water in pipes.

CHAPTER 9. Of the motion of water in pipes.

The author has attempted to ſimplify this ſubject nearly in the ſame manner as that of the motion of rivers, and apparently with conſiderable ſucceſs. He obſerves, that the head of water may be divided into two parts, one of which is employed in producing velocity, the other in overcoming the friction: that the height employed in overcoming the friction muſt be as the length of the pipe directly; and alſo directly as the circumference of the ſection, or as the diameter of the pipe, and inverſely as the content of the ſection, or as the ſquare of the diameter; that is, on the whole, inverſely as the diameter; this height too muſt vary, like the friction, as the ſquare of the velocity.

To find the velocity;

Hence $f = \frac{al}{d}.v^2$, f denoting the height due to the friction, and a a conſtant quantity: therefore, $v^2 = \frac{fd}{al}$. Now the height employed on the friction, correſponds to the difference between the actual velocity and the actual height, or $f = h - \frac{v^2}{b^2}$, where b is the coefficient for determining the velocity from the height; conſequently, $v^2 = \frac{b^2dh - dv^2}{ab^2l}$, and $v^2 = \frac{b^2dh}{ab^2l + d}$. Now $b = 6.6$, and from Buat's experiments, ab^2 is determined to be .0211, which agrees the moſt accurately where the velocity is between 6 and 24 inches in a ſecond. Whence we have

$v^2 = \frac{43.6dh}{.0211l+d}$, or $v = 45.5 \surd\left(\frac{dh}{l+47d}\right)$; but it is ſomewhat more accurate to make $v = 50 \surd\left(\frac{dh}{l+50d}\right)$; all the meaſures being expreſſed in Engliſh feet.

allowance for flexure.

When the pipe is bent into angles, or rather arcs, we muſt diminiſh the velocity thus found, by taking the product of its ſquare multiplied by the ſum of the ſines of the ſeveral angles of inflection, and then by .0038; which will give the degree of preſſure employed in overcoming the reſiſtance occaſioned by the angles: and deducting this height from the height correſponding to the velocity, we may thence find the corrected velocity.

Compound pipes.

Mr. Eytelwein proceeds to inveſtigate, both theoretically and experimentally, the diſcharge of water by compound pipes, with apertures of various dimenſions between them: he allows at each orifice for the contraction of the ſtream, and calculates the height neceſſary to produce the increaſe of velocity in each inſtance, allowing alſo for the friction of the pipe. But the velocity thus found is ſomewhat ſmaller than the reſult of his experiments; probably becauſe the whole of the force of the water accelerated at any orifice, is not immediately loſt as ſoon as it arrives at a wider part of the pipe. The aſcent of water in a compound pipe, to the level of a reſervoir, is next conſidered, a caſe which often occurs in pump-work; and an approximation to the velocity of aſcent is deduced from theory and compared with experiment.

Jets of water.

Chapter 10. Of jets of water.

This chapter contains little that is new or intereſting; it is well known, that the velocity of a jet is greateſt when it ſprings through an orifice in a thin plate, and in this caſe, the height falls little ſhort of that of the reſervoir.

Impulſe or hydraulic preſſure of water.

Chapter 11. Of the impulſe or hydraulic preſſure of water.

There are three principal caſes of the impulſe of water falling perpendicularly on plane ſurfaces: when a detached jet of water ſtrikes the plane; when the plane moves in an unlimited extent of water, or is very ſmall in reſpect to a ſtream that ſtrikes it; and when the impulſe takes place in a limited channel.

When the ſtream ſes all its velo- ity;

Suppoſing a ſtream of water to ſtrike againſt a plane ſo as to loſe all its motion, it is obvious, that the force that deſtroys the motion muſt be equal to the force that generates it; that is, to the weight of the column of water operating during the time neceſſary for its acquiring the given velocity: and the quantity of water arriving during this time, being equal to twice the column of which the length is the height due to the velocity, the hydraulic preſſure muſt be twice the weight of ſuch a column. The relative impulſe againſt a plane in motion, muſt be determined from the difference of the velocities: but when all the water of a ſtream ſtrikes againſt a plane, the effect of the impulſe may be more ſimply determined, as if a ſolid body ſtruck the plane with the relative velocity; and this is nearly what happens in underſhot water-wheels.

or when the plane recedes.

Detached jet.

When a detached jet ſtrikes againſt a plane, it appears, from the experiments of Boſſut and Langſdorf, that its effect is equal to the weight of an equal column of twice the height due to the velocity; but the plane muſt be at leaſt four times as large in diameter as the jet; if it be only of the ſame ſize, the effect will be but one half as great. In an unlimited ſtream, the impulſe is alſo nearly determined from the height correſponding to the velocity; and it appears, that the effect is nearly doubled by confining the ſtream to prevent its diverting laterally from the float-boards.

Unlimited ſtream;

confined in a channel.

Oblique ſurfaces oppoſed to a jet.

For oblique ſurfaces, the effect of a detached jet in its own direction, appears to vary as the ſquare of the ſine of the angle of incidence; but, for motions in open water, we muſt add to this ſquare about $\frac{2}{5}$ of the difference of the ſine from the radius: a correction which is tolerably accurate, until the inclination becomes very great. Mr. Eytelwein found the reſiſtance to the motion of a ſphere nearly $\frac{4}{5}$ of the reſiſtance to a circle equal to its ſection: perhaps it was a hemiſphere, otherwiſe it is difficult to reconcile the reſult with other experiments in which it has appeared to be only $\frac{2}{5}$.

Weight of diſtilled water.

Mr. Eytelwein informs us, that at the temperature 14° of Reaumur, or 63½ of Fahrenheit, a cubic foot of diſtilled water weighs 66.0656 pounds of Cologne, or 65.9368 commercial pounds of Berlin. According to Sir George Shuckburgh's experiment, an Engliſh cubic foot of diſtilled water at 66° weighs 997 ounces avoirdupois; and water expands for every degree .000165: hence the pound of Cologne is 1.0312 Engliſh avoirdupois pounds, and that of Berlin 1.0332.

Overſhot wheels;

Chapter 12. Of overſhot water-wheels.

The power which operates upon overſhot wheels, is divided into two parts, one derived from the weight of the water in the cells or buckets, the other from the impulſe of the water falling on it: the effect of the firſt is conſtant, that of the ſecond varies with the velocity: the maximum is found to be when the velocity is half that of the water received; but the variable part being the ſmaller, the rule is of little practical conſequence, and the velocity of the wheel is generally greater than this. The author obſerves, that by turning the ſtream back upon the nearer half of the wheel, we remove the reſiſtance of the lower water, ſince it runs off in the ſame direction with that of the water-wheel.

advantage of reverſing their rotation.

Underſhot wheels.

Chapter 13. Of underſhot water-wheels.

Floatboards ſhould riſe perpendicularly.

Underſhot wheel ought to work with half the velocity of the ſtream.

The author enters into a minute deſcription of the parts of an underſhot water-wheel: he obſerves, that the moſt advantageous poſition for the floatboards in a ſtraight channel is, when they are perpendicular to the water at the time that they riſe out of it: that only one half of each ſhould ever be below the ſurface, and that from three to five ſhould be immerſed at once, according to the magnitude of the wheel. When there is ſufficient fall, the floatboards ſhould be divided and made into buckets, ſo that the wheel may become a breaſt wheel; the poſition of the external portion being ſuch, that a line drawn through it at the time when the water enters, may divide the vertical radius in the ſame proportion that it divides the quadrant of the circumference; that is, if the water is received, for inſtance, at one third of the quadrant from the bottom, the line muſt leave one third of the radius above it. A formula is laid down for calculating the actual force of a given ſtream of water on a wheel, and it is ſhown, that half the velocity of the ſtream is that which gives the maximum of effect, the theory agreeing perfectly with the experiments of Smeaton and others: for, ſince the effect is eſtimated by the product of the force into the velocity of the parts upon which it acts, and ſince the force is in this caſe ſimply as the relative velocity, becauſe the quantity of water is given, and the whole of it is ſuppoſed in all caſes to act; therefore, the effect will be expreſſed by the product of the relative and abſolute velocity of the wheel, or $e=ra$; but $r=v-a$, v being the velocity of the ſtream, and $ra=av-aa$, which is obviouſly greateſt when $v=2a$, as is evident either by taking the fluxion, or by conſidering that the greateſt ordinate of a ſemicircle is the radius.

To ſhow the advantage of breaſt wheels over common underſhot wheels, the author quotes Mr. Banks's experiments. He alſo obſerves, that by placing two wheels after each other in the ſame ſtream, about one fourth more force may be obtained than either by a ſingle wheel, or by two wheels ſide by ſide; but that a ſingle wheel has leſs friction, and is generally leſs expenſive.

Breaſt wheels much better than underſhot.

Chapter 14. Of the properties of the air, as far as they relate to hydraulic machines.

What Mr. Eytelwein quotes as Mariotte's diſcovery of the increaſe of the air's denſity in proportion to the preſſure, was well known to Hooke and Boyle. From the experiments of Woltmann and Schober, he remarks, that the force of the wind againſt a perpendicular plane, is nearly equal to four thirds of the weight of a column of air, of a length equal to the height due to the velocity. The height of a column of water nearly equivalent to the force or reſiſtance may be found, by taking the ſquare of $\frac{1}{200}$ of the velocity in a ſecond, in Engliſh feet.

Force of air againſt a plane;

Rule for finding it.

Thus, if the velocity were 1000 feet in a ſecond, the reſiſtance would be equal to a column of water in the ſame ſurface, 25 feet in height; and the reſiſtance to a ſphere about half as much.

Example.

For another example, if we had a cubic foot of a ſubſtance equal in ſpecific gravity to water, and were deſirous of knowing the greateſt velocity that it could acquire by falling through the air; the height of the column of water is here 1, and its ſquare root 1, which multiplied by 200 gives 200 feet in a ſecond for the velocity, when the reſiſtance would be equal to the weight, which of courſe is the limit beyond which the velocity could never paſs. Hence we may form an idea of the utmoſt velocity that a ſtone, of moderate ſize, could acquire in deſcending from the upper regions of the atmoſphere, or even from the neighbourhood of the moon; a velocity that would be much leſs than that of a bomb or a cannon ball, even when it may be followed by the eye.

Inſtance of the utmoſt velocity poſſible to be had by a body falling in the air:

much leſs than that of a cannon ball.

Again, Mr. Garnerin's parachute contains about 860 ſquare feet of ſurface, and weighs, together with the aeronaut ſuſpended from it, about 230 pounds. Here the weight is $\frac{23}{86}$ of a pound for each ſquare foot, which is equivalent to $\frac{1}{230}$ of a foot of water; multiplying the ſquare root by 200, we have about 13 feet in a ſecond for the utmoſt velocity; which is the

Parachute of Garnerin.

ſame as if one leaped from a height of between two and three feet. Mr. Garnerin, however, finds the mean velocity of deſcent only eight feet, which agrees better with the experiments of Borda, in which the reſiſtance appeared to be $\frac{5}{3}$ of the weight of the column due to the velocity, and exceeded this proportion as the ſurface increaſed in magnitude.

Syphons.

Chapter 15. Of ſiphons.

For eſtimating the diſcharge of a ſiphon, the head of water muſt be reckoned equal to the difference between the levels of the ſurface of the water, and of the lower orifice. The author obſerves, that the theory of waves has been treated in a new and improved manner by Lagrange in his Mécanique Analytique. The problem is, however, not yet completely ſolved: Lagrange's formula includes the depth of the water agitated as a given quantity, but it does not inform us how to determine this depth from theory.

Sucking pumps.

Chapter 16. Of ſucking pumps.

The length of a ſucking pump muſt never be greater than 30 feet below the moveable valve: and there may be a loſs of time in the aſcent of the water, unleſs it be made even a few feet ſhorter. The motions to be produced, and the reſiſtances to be overcome, are conſidered in detail: but the author refers, for ſtill further information, to Langſdorf's Treatiſe on Machinery.

Nature of the ſtroke.

The velocity of the ſtroke ſhould never be leſs than four inches, nor greater than two or three feet in a ſecond; the ſtroke ſhould be as long as poſſible, to prevent loſs of water by the frequent alternations of the valves. The diameter of the pipe ſhould be about $\frac{2}{3}$ or $\frac{3}{4}$ of that of the barrel. The lifting pump is alſo here deſcribed; it only differs from the ſucking pump in having the lower valve moveable, and the upper one fixed. A number of valves and piſtons are deſcribed in this chapter; chiefly from models of Engliſh manufactory.

Forcing pumps;

Chapter 17. Of forcing pumps.

In deſcribing the different kinds of ſolid piſton, the author gives the preference to that which has a conical leather projecting on each ſide; but remarks, that there is another form which has the advantage in the ſituation of the ring for receiving the rod, which is preciſely in the centre of the piſton, and is therefore fitter for communicating motion in each direction. He ſays, that where the barrel is well poliſhed, the piſton may be uſed without either wadding or leather.

made without leathering.

The firſt pump was a forcer

The firſt pump, invented above a century before Chriſt, by Cteſibius of Alexandria, to whom alſo muſic is indebted for the organ, and whoſe name Mr. Eytelwein mentions in ſpeaking of ſucking pumps, was in reality a forcing pump, as may eaſily be collected from its deſcription by Vitruvius (L. X. cap. 12.)

Mixed pumps.

Chapter 18. Of mixed pumps, or the combination of ſucking and forcing pumps.

When the lower valve is above the ſurface of the water, the forcing pump can only raiſe the water by ſuction, but the conſtruction remains the ſame. Such is Mr. Buchanan's patent ſhip pump. De la Hire's pump is more complicated; both the aſcending and deſcending ſtrokes of the piſton being made effective, by means of a double apparatus of valves and pipes.

Included columns acting by preſſure.

Chapter 19. Of acting columns of water.

The mechaniſm of a pump may be employed for converting the weight of water deſcending in its barrel, to the purpoſe of working another pump. The author deſcribes a machine of this kind invented by Mr. Höll, and improved by Langſdorf. A ſimilar arrangement, uſed in Cornwall, has lately been deſcribed in Nicholſon's Journal, by Mr. Trevithick. The only objection to it appears to be the magnitude of the friction.

The ſpiral pump or horizontal helix forcing water up a pipe.

Chapter 20. Of the ſpiral pump.

If we wind a pipe round a cylinder, of which the axis is horizontal, and connect one end with a vertical tube, while the other is at liberty to turn round and receive water and air in each revolution, the machine is called a ſpiral pump. It was invented about 1746, by Andrew Wirz, a pewterer in Zurich, and was employed at Florence with Bernoulli's improvement, in 1779. At Archangelſky, near Moſcow, a pump of this kind was erected in 1784, which raiſed a hogſhead of water in a minute, to a height of 74 feet, and through a pipe 760 feet in length. The force employed is not mentioned, we may therefore conjecture that it was turned by water. Mr. Eytelwein enters very minutely into calculations of the effect of ſuch a machine under different circumſtances; and the reſults of theory, as well as of experiment, are ſuch, as to induce us to expect that it will in time come into common uſe, inſtead of forcing pumps of a more complicated and expenſive conſtruction. The water-tight joint preſents the only difficulty: the pipe may form either a cylindrical, a conical, or a plane ſpiral, and it appears to be uncertain which is the moſt advantageous: the vertical pipe ſhould be nearly of the ſame dimenſions as the ſpiral pipe, which may without difficulty be made of wood.

Screw of Archimedes.

Chapter 21. Of the ſcrew of Archimedes, or the waterſnail, and of the waterſcrew.

The ſcrew of Archimedes conſiſts either of a pipe wound ſpirally round a cylinder, or of one or more ſpiral excavations, formed by means of ſpiral projections from an internal cylinder, covered by an external coating ſo as to be water-tight. But if the coating is detached, ſo as to remain at reſt while the ſpirals revolve, the machine is called a waterſcrew. Mr. Eytelwein enters into an accurate determination of the effects of theſe machines in given circumſtances, and the reſults of the theory agree remarkably well with experiment. He obſerves, that the ſcrew of Archimedes ſhould always be ſo placed, as to fill exactly one half of a convolution in each turn; and that very unfavourable reports have ſometimes been made of the machine from want of attention to this circumſtance; for when the orifice remains conſtantly immerſed, the effect is very much diminiſhed: this appears alſo to have happened in ſome late experiments in London. Where the height of the water is ſo variable as to render this precaution impoſſible, Mr. Eytelwein prefers the waterſcrew; although, in this inſtrument, one third of the water generally runs back, and it is eaſily clogged by accidental impurities in the water. The ſcrew of Archimedes is generally placed ſo as to form an angle of between 45° and 60° with the horizon; but the open waterſcrew at an angle of 30° only: for great heights, the ſpiral pump is preferable to either.

The waterſcrew.

Caution for placing the ſcrew of Archimedes.

Bucket wheels.

Chapter 22. Of bucket wheels and throwing wheels.

In the conſtruction of wheels for raiſing water in buckets, there is little room for refined theory; whether the buckets be fixed or ſuſpended on an axis. It is ſometimes convenient to raiſe water to the height of 3 or 4 feet by the revolution of a wheel with ſimple floatboards; and ſuch a wheel may be either in a vertical or an inclined poſition; it muſt of courſe be incloſed in a ſweep.

Throwing wheel.

Chapter 23. Of cellular pumps and paternoſter works.

Cellular pumps or paternoſter works.

The chain-pump.

Water has been ſometimes raiſed by ſtuffed cuſhions connected with an endleſs rope, and cauſed by means of two wheels or drums to riſe in ſucceſſion in the ſame barrel, and to carry water with them: but the magnitude of the friction appears to be an objection. From the reſemblance of the apparatus to a ſtring of beads, it has been called a paternoſter work. When flat boards are united by chains, and employed inſtead of theſe cuſhions, the machine may, without impropriety, be called a cellular pump: here the barrel is generally ſquare, and placed in an inclined poſition. But theſe machines are very rarely employed.

Chapter 24. Of inſtruments for meaſuring the velocity of ſtreams of water.

Inſtruments for meaſuring the velocity of ſtreams of water.

The ſuperficial velocity of a ſtream is aſcertained without difficulty, by obſerving in calm weather the motion of a body barely floating on it. But it is more difficult to determine the velocity of a river at a conſiderable depth. Pitot's tube, as improved by Buat, furniſhes one of the eaſieſt methods. A funnel is preſented to the ſtream, and the water in a vertical tube connected with it, is elevated above the level of the river, nearly to the height correſponding to the velocity: but the reſult will be more accurate, if the funnel be covered by a plate perforated only in the centre by a ſmall orifice: in this caſe, the elevation in the tube will be half as great again as the height due to the velocity. Other inſtruments for appreciating the impulſe of the water againſt a flat board, require ſome previous comparative obſervation. In Woltmann's hydrometrical fly, the number of revolutions of a wheel, in a given time, indicates the velocity of the water, which ſtrikes againſt two inclined planes, and carries round the arm to which they are fixed.

Concluding addreſs.

It is preſumed, that this abridged account of Mr. Eytelwein's book, will not only do juſtice to his diligence and ingenuity, but will convey to the Engliſh reader ſome matter perfectly new, and capable of frequent application in practical hydraulics; which is perhaps of the more value, as there is little probability that the work will be tranſlated at length. To diſſeminate information of this kind muſt always be the principal object of the Journals of the Royal Inſtitution.

Y.

IV.

Experiments and Obſervations on certain ſtony and metalline Subſtances, which at different Times are ſaid to have fallen on the Earth; alſo on various Kinds of native Iron. By Edward Howard, *Eſq. F. R. S. From the Philoſophical Tranſactions,* 1802.

(Continued from Page 263, Vol. II.)

Chemical examination of ſtones fallen on the earth.

I PROCEED to conſider the aſſiſtance to be derived from chemiſtry in diſtinguiſhing theſe ſtones from all other known ſubſtances, and in eſtabliſhing the aſſertion, that they have fallen on the earth.

Objections to the analyſis of the ſtone preſented by the Abbé Bachelay,

The analyſis made by the French academicians of the ſtone preſented to them by the Abbé Bachelay, was in part conducted by the ever to be deplored Lavoiſier; but it was performed before that celebrated author had enriched chemiſtry with his laſt diſcoveries, and before he had given birth to the ſyſtem under which it flouriſhes. The reſult of this analyſis might well induce the concluſion, that the ſubject of it was common pyritical matter. It was unfortunately made of an aggregate portion of the ſtone, and not of each diſtinct ſubſtance irregularly diſſeminated through it. The proportions obtained were, conſequently, as accidental as the arrangement of every ſubſtance in the maſs.

and to that of M. Barthold.

The analyſis of M. Barthold, of the ſtone of Enſiſheim, is ſubject to the ſame objections: but, after having the advantage of the foregoing deſcriptions, the reſearches which follow cannot be ſuppoſed altogether liable to a ſimilar fatality.

EXAMINATION OF THE STONE FROM BENARES.

Chemical examination of the ſtone from Benares.

This ſtone, as the Count de Bournon has already remarked, has the moſt diſtinguiſhed characters. Indeed it is the only one of the four ſufficiently perfect (if I be allowed that expreſſion) to be ſubjected to any thing approaching to a regular analyſis.

The black coating deprived of magnetic parts

The cruſt, or external black covering, is the firſt ſubſtance to which the attention is naturally directed. When a portion of this cruſt had been detached with a knife or a file, and finely pulverized, I ſeparated the particles attractable by a magnet, and digeſted the unattractable portion with nitric acid, which was preſently decompoſed; but, owing to a ſtrong adherence of ſome of the interior and earthy parts of the ſtone, it did not diſentangle the coating or metalline part without ſome difficulty. The acid being ſufficiently neutralized, the ſolution was paſſed through a filter, and ſaturated to exceſs with ammonia. An abundant precipitate of oxide of iron was produced; and, when this oxide was ſeparated, I obſerved the ſaline liquor to have a greeniſh colour. I evaporated it to dryneſs, and rediſſolved the dry ſalt in diſtilled water. No precipitate was formed during the evaporation, nor was the colour of the ſolution entirely deſtroyed. It appeared to me like a triple ſalt, deſcribed by Mr. Hermſtadt[*] as an ammoniacal nitrate of nickel. By examination with pruſſiate of ammonia, it yielded a whitiſh precipitate, inclining to a violet colour; and, by various properties, I was ſoon confirmed in the opinion, that nickel was preſent. Since I ſhall have occaſion more than once to treat of the triple compound, and ſince it has been only mentioned by Mr. Hermſtadt, it is neceſſary now to detail ſome of its diſtinctive characters. The ſame chemiſt informs us, that the three mineral acids, with ammonia, enter into ſimilar combinations with nickel; and I have obſerved, that oxide of nickel can be diſſolved by nitrate and muriate of ammonia. The muriate ſeems to take up the largeſt quantity. The colour of this ſalt is by no means uniform: it is ſometimes graſs green, violet, roſe colour, inclining to purple, and I have ſeen it almoſt colourleſs. It ſeems to be purple, and to incline to roſe colour and violet, when all the oxide of nickel is not united to both acid and alkali, but, from the deficiency of ſalt, is held in ſolution by an exceſs of ammonia. In this caſe, evaporation, of courſe, precipitates the nickel in the ſtate of oxide, which is of a whitiſh green colour.

was digeſted in nitric acid; which it decompoſed.

The iron ſolution was precipitated by ammonia.

Ammoniacal nitrate of nicke remained.

Diſtinctive characters of this triple ſalt.

The nickel cannot be *precipitated* from a perfectly formed triple ſalt, by any reagent I have tried, except by a pruſſiate, or a hidrogenized ſulphuret of ammonia. Potaſh and lime, as

The nickel is precipitable by a pruſſiate or an hidrogenized ſulphuret of ammonia.

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[*] Annales de Chimie, Tom. XXII. p. 108.

well as, I preſume, other bodies, ſtanding in the order of affinities before ammonia, decompoſe the ſalt; but the nickel is then continued in ſolution by the diſengaged ammonia.

The liquid was always examined to detect copper if it had been preſent.

As it may be imagined that I have occaſionally met with copper, when I deſcribe a violet or purple ammoniacal ſolution, it is right to obſerve, that to avoid this error, I have either reduced the liquor to a neutral ſtate, and endeavoured, without ſucceſs, to obtain from it a precipitate, with a ſolution of ſulphureted hidrogen gas; or, by adding an acid to ſlight exceſs, and immerſing a piece of iron, I have not been able to detect a trace of copper. Theſe, and many other trials, when they do not appear to be made before the eſtimation of the quantities of nickel, have been conſtantly made afterwards.

The coating contained iron nearly metallic, and ſome nickel.

But, to return to the incruſtation or coating of the ſtone, the decompoſition of the nitric acid ſhewed the preſence of matter at leaſt nearly metallic, although not attractable; and the examinations made of the liquor, from which the iron was precipitated, aſcertained the preſence of nickel beyond diſpute. The difficulty of obtaining the coating of the ſtone, either diſtinct from matter not belonging to it, or in ſufficient quantity, induced me to relinquiſh the idea of attempting to give the proportions of its conſtituent parts.

The ſhining particles of the ſtone

The ſtone being deprived of its covering, the ſhining particles irregularly diſſeminated, next demand examination. I firſt examined the pyrites. Their very looſe texture made it exceedingly difficult to collect the weight of 16 grains, which was however effected by the dexterity of the Count de Bournon.

were digeſted in dilute muriatic acid.

I digeſted theſe, at a low heat, with weak muriatic acid; which acted gradually, and diſengaged a trifling but ſenſible quantity of ſulphureted hidrogen gas. After ſeveral hours, I found the acid diſcontinued its action. The whole metalline part appeared in ſolution; but ſulphur and earthy particles were obſervable. The ſulphur, from its ſmall ſpecific gravity, was ſuſpended through the ſolution; whilſt the earthy matter, which could not be ſeparated by mechanical means, was fortunately left at the bottom of the digeſting veſſel. I decanted off the ſolution, holding ſuſpended the ſulphur; and, by repeated waſhing, ſeparated every thing belonging to the pyrites,

It diſſolved the metal, and ſuſpended the ſulphur, and the earthy portion ſubſided.

rites from the inſoluble earthy matter, the ſubtraction of which reduced the weight of real pyrites to 14 grains. I next obtained the ſulphur, by filtration. When it was as dry as I could make it, without ſear of its being ſublimed, its weight was two grains. To the filtrated liquor I added nitrate of barytes, by way of detecting any ſulphuric acid which might have been preſent; but no cloudineſs enſued. I then ſeparated, by ſulphate of ammonia, the barytes thus added, and precipitated the iron with ammonia. The liquor, on the ſubſidence of oxide of iron, appeared of a violet purple colour: it contained nickel, which I threw down with ſulphureted hidrogen gas, there being already a ſufficient exceſs of ammonia in the ſaline liquor to form an alkaline hidrogenized ſulphuret. The oxide of iron, after ignition, weighed 15 grains; and the ſulphuret of nickel, reduced to an oxide, weighed, after the ſame treatment, ſomething more than one grain. The proportions of the ſubſtances contained in the pyrites of the ſtone from Benares, may therefore be conſidered nearly thus.

The metals were iron and ſome nickel;

	Grains.
Sulphur - - - - - -	2
Iron - - - - - -	10½
Since 15 grains of the oxide repreſent about that quantity of iron,	
Nickel, nearly - - - -	1
Extraneous earthy matter - -	2
	15½

Component parts, ſulphur, much iron, nickel, and earth.

It is obſervable that, notwithſtanding the loſs appears to be only half a grain, it was probably more, becauſe the ſulphur could not be reduced to the ſame ſtate of dryneſs in which it exiſted when in combination with the iron; not to ſay that it was, in a ſmall degree, volatilized with the hidrogen gas diſengaged during the ſolution.

Remarks.

The weight of nickel is a mere eſtimation. We are not yet ſufficiently acquainted with that metal to ſpeak of it with accuracy, except as to its preſence. Upon the whole, however, it may be concluded, that theſe pyrites are of a very particular nature; for, although Henkel has obſerved that ſulphur may be ſeparated from pyrites by muriatic acid, it is by no means the uſual habitude of pyrites to be of ſuch eaſy decompoſition.

The other ſhining particles being malleable iron,

The other ſhining particles immediately ſeen, when the internal ſtructure of the ſtone is expoſed, are the malleable iron. Before I ſtate the examination of this iron, I muſt remark, that preliminary experiments having ſhewn me it contained nickel, I treated ſeveral kinds of the moſt pure irons I could obtain, with nitric acid; and precipitated the oxide from the metallic ſalt by ammonia. The quantity of oxide I obtained from 100 grains of iron, was from 144 to 146. I may conſequently infer, that 100 grains of pure iron acquires, by ſuch a proceſs, 45 grains of oxigen; and that, whenever a metallic ſubſtance, ſuppoſed to be iron, does not, under the ſame circumſtances, acquire the ſame proportionate weight, ſomething is either volatilized, or left in ſolution. Hence, when a metallic alloy of nickel and iron preſents itſelf, a judgment may, at leaſt, be formed of the quantity of nickel, by the deficiency of weight in the precipitated oxide of iron.

were diſſolved in nitric acid, and precipitated by ammonia,

This mode of treatment was not allowed me in the examination of the coating of the ſtone, becauſe it was impoſſible to know in what ſtate of oxidizement the iron exiſted. But, as the particles diſſeminated through the whole maſs, are clearly metallic, a very tolerable idea of the quantities of nickel contained in them will be obtained, by noting the quantity of oxide of iron ſeparated, as above deſcribed. 25 grains of theſe metallic particles were therefore heated with a quantity of nitric acid, much more than ſufficient to diſſolve the whole. Some earthy matter, which, as in a former caſe, was not ſeparable by mechanical means, remained after a complete ſolution of the metal had been effected. This earthy matter, after being ignited, weighed two grains. The real matter of the preſent examination, was therefore reduced to 23 grains, and was in complete ſolution. I added ammonia to a very ſenſible exceſs. The oxide of iron was thereby precipitated, and, being collected and ignited, it weighed 24 grains; whereas, according to my experiments, 33½ grains ſhould have been produced from the ſolution, had it contained nothing but iron. I examined the ſaline liquor, when free from ferruginous particles, and diſcovered it to be the triple ſalt of nickel. Hence, allowing for loſs, the quantity of nickel may be eſtimated, by calculating the quantity of iron contained in 24 grains of oxide. Thus, if 145 grains of oxide contain 100 of iron, about 16½ are contained in 24 of oxide. This would ſuppoſe the 23 grain

which left nickel in triple ſolution of which the quantity was eſtimated.

grains of alloy to conſiſt of 16½ iron and 6½ nickel; which, if the uſual loſs be added to the 16½ grains of iron, and deducted from the nickel, may not be very remote from the truth.

The globular bodies diſperſed through the ſtone, exhibited nothing magnetic.

I ſhall next examine the globular bodies, alſo irregularly diſperſed throughout the ſtone. A number of them were reduced to fine powder; but nothing metallic could be ſeparated by the magnet. As a preliminary experiment, I ſought for pyrites, by digeſtion with muriatic acid; but no hepatic ſmell was in the leaſt perceivable, nor was white carbonate of lead at all altered by being held over the mixture. I therefore conclude theſe globular bodies do not envelope either iron or pyrites. By way of analyſis, I treated 100 grains with potaſh, in a ſilver crucible; and, after the uſual application of a red heat, ſeparated as much ſilica as poſſible, by muriatic acid and evaporation. The ſilica being collected on a filtre, carbonate of potaſh was added to the filtrated liquor; by which, a precipitate, almoſt wholly ferruginous, was produced. This precipitate was collected in the common way; then boiled with potaſh, to extract alumina; and, by ſuperſaturating the alkaline liquor with muriatic acid, and precipitating by carbonate of ammonia, an earth was gathered, which I afterwards found to be partly, if not intirely, ſiliceous. After rediſſolving, in muriatic acid, the portion of the ferruginous matter rejected by the potaſh, I precipitated by ammonia, what I took to be intirely oxide of iron; but, after igniting it, and again attempting to rediſſolve the whole in muriatic acid, more ſilica was left. The non-exiſtence of lime was proved, by the addition of carbonate of ammonia, immediately after the ſame alkali, pure, had thrown down what I took wholly for oxide of iron. I had now obtained every thing in the ſubject of my analyſis, except magneſia and nickel. The former, and a trace of the latter, were held by carbonic acid in the liquor, from which the ferruginous precipitate was, in the firſt inſtance, thrown down by carbonate of potaſh; and the latter was found in the laſt named muriate of ammonia. I diſengaged the magneſia, by the aſſiſtance of potaſh, and by evaporating to dryneſs. The oxide of nickel was precipitated by hidrogenized ſulphuret of ammonia.

Analyſis.

Conſtituent parts.

Under all circumſtances, I am induced to ſtate the proportions of conſtituent parts thus:

Silica

Silica	50
Magneſia	15
Oxide of iron	34
Oxide of nickel	2½
	101½

Remarks.

The exceſs of weight, inſtead of the uſual loſs, is owing to the difference of oxidizement of the iron, in the ſtone and in the reſult of the analyſis; which will be found to be the caſe in all analyſes of theſe ſubſtances; indeed it is always neceſſary to reduce the oxide to the red ſtate, as being the only one to be depended upon. To avoid future repetition, I ſhall alſo obſerve, firſt, that by preliminary experiments, I could not detect any other ſubſtance than thoſe mentioned. Secondly, that the earth obtained as alumina, appeared to me to be moſtly, if not intirely, ſiliceous; becauſe, after it had been ignited, and again treated with potaſh and muriatic acid, I found it was very nearly all precipitated by evaporation. Thirdly, I examined, and judged of, the ſilica collected from the oxide of iron, in the ſame way. Fourthly, the weight of the magneſia is given, not immediately, as obtained by evaporation, but after a ſubſequent ſolution in an acid, and precipitation by potaſh. And, fifthly, the proportions are taken from the mean of two analyſes.

The earthy matrix or cement.

Nothing remains to be examined, of the ſtone from Benares, except the earthy matter, forming a cement or matrix for the ſubſtances already examined. 100 grains of this matter were, by mechanical means, ſeparated as perfectly as poſſible, from the pyrites, iron, and globular bodies, and analyſed as above. The mean reſult of two analyſes gave,

Its component parts.

Silica	48
Magneſia	18
Oxide of iron	34
Oxide of nickel	2½
	102½

Stone from Sienna.

EXAMINATION OF THE STONE FROM SIENNA.

The external coating of this ſtone appeared to have the ſame characters as that of the ſtone from Benares.

The pyrites, although certainly preſent, were not cryſtallized in ſuch groups as in the preceding ſtone; nor could they be ſeparated by mechanical means.

Iron and nickel as in the former inſtance from the attractable metal.

The attractable metal was eaſily ſeparated by the magnet; but 8½ grains only were collected. I treated them with nitric acid and ammonia, as in a preceding caſe. Nearly one grain of earthy matter was inſoluble; the weight was therefore reduced to rather leſs than 8 grains. The oxide of iron, precipitated by ammonia, weighed 8 grains; and the ſaline liquor gave abundant indications of nickel. As 8 grains of this oxide of iron contain nearly 6 of metal, the quantity of nickel, in the bare 8 grains, may be eſtimated between 1 and 2 grains. Some globular bodies were extracted, but too few to analyze.

Analyſis of the ſtone freed from magnetic metal and globular parts.

Since the pyrites could not be ſeparated, I collected 150 grains of the ſtone, freed from iron by the magnet, and as exempt as poſſible from globular bodies. Theſe 150 grains, I firſt digeſted with muriatic acid, that the pyrites might be decompoſed, and every thing taken up which could be diſſolved by that menſtruum. A very decided diſengagement of ſulphureted hidrogen gas was occaſioned. When the acid could produce no further action, I collected the undiſſolved matter on a filtre, and boiled it with the moſt concentrate nitric acid, in hopes of being able to convert the ſulphur, previouſly liberated, into ſulphuric acid; but my endeavours were fruitleſs; for, upon the addition of nitrate of barytes to the nitric ſolution, rendered previouſly tranſparent, a very inſignificant quantity of ſulphate of barytes was obtained. The ſurplus of barytic nitrate was removed by ſulphate of potaſh. I next completely edulcorated the maſs which remained inſoluble, after the action of the muriatic and nitric acids; and, adding the water of edulcoration to the muriatic and nitric liquors, evaporated the whole for ſilica. I then ſubmitted the maſs, undiſſolved by the acids and the water, to the treatment with potaſh, muriatic acid, and evaporation, which was, in the firſt inſtance, applied to the ſtone from Benares. The firſt precipitation was, as in that analyſis, alſo effected with carbonate of potaſh; but, inſtead of endeavouring immediately to extract alumina, I ignited the precipitate, that the alumina or ſilica remaining might be rendered inſoluble. After the ignition, I ſeparated the oxide of iron with very concentrate muriatic acid; and the earths, which were left perfectly white, I heated

I heated with potaſh, until they were again capable of being taken up by the ſame acid. The ſolution ſo made, was ſlowly evaporated; and, as very nearly every thing was depoſited during the evaporation, I conclude all was ſilica. The proportions reſulting from this ſingle analyſis, without the weight of ſulphur contained in the pyrites irregularly diſſeminated through the whole, were,

Its component parts.

Silica	70
Magneſia	34
Oxide of iron	52
Oxide of nickel	3
	159.

The ſtone from Yorkſhire.

EXAMINATION OF THE STONE FROM YORKSHIRE.

The mechanical ſeparation of the ſubſtances in this ſtone being as difficult as in the preceding caſe, I was neceſſarily ſatisfied with ſubmitting it to the ſame treatment. I collected, however, 34 grains of malleable particles; which, by the proceſs already more than once mentioned, left 4 grains of earthy matter; and, by yielding 37½ of oxide of iron, indicated about 4 grains of nickel.

Component parts.

150 grains of the earthy part of the ſtone were, by analyſis, reſolved into,

Silica	75
Magneſia	37
Oxide of iron	48
Oxide of nickel	2
	162.

The ſtone from Bohemia.

EXAMINATION OF THE STONE FROM BOHEMIA.

The probability of never being able to obtain another ſpecimen of the very remarkable fragment of this ſubſtance, did not allow me to treſpaſs more on the liberality of Mr. Greville, than to detach a ſmall portion. I found it of ſimilar compoſition to that of the three preceding ſtones; and the Count de Bournon has already ſhewn the proportionate quantity of the attractable metal to be very conſiderable. 16½ grains, left 2½ of extraneous earthy matter; and yielded, by the treatment with nitric acid and ammonia, 17½ grains of oxide of iron. This would ſeem to iuduce an eſtimation of 1½ of nickel in 14 grains, or about 9 per cent.

Component parts.

55 grains of the earthy part of the ſtone, by the analytical treatment of the two former, afforded,

Silica - - - -	25
Magneſia - - - -	9½
Oxide of iron - - -	23½
Oxide of nickel - - -	1½
	59½

The unuſual increaſe of weight in the reſult of the three laſt analyſes, notwithſtanding the intire loſs of the ſulphur in the pyrites, is obviouſly owing to the metallic ſtate of the iron combined with the ſulphur, as was ſhewn in a former inſtance.

Remarks on theſe analyſes: compared with thoſe formerly made by others.

I have now concluded the chemical examination of theſe four extraordinary ſubſtances. It unfortunately differs from the analyſis made by the French Academicians, of the ſtone preſented to them by the Abbé Bachelay, as well as from that made by Profeſſor Barthold, of the ſtone of Enſiſheim. It is at variance with that of the Academicians, inaſmuch as they found neither magneſia nor nickel. It differs from that of Mr. Barthold, as he did not find nickel, but diſcovered ſome lime, with 17 per cent. of alumina. With regard to theſe differences, I have to ſubmit to the chemical world, whether magneſia might not have eluded the action of an acid, when the aggregation of the integrant parts of the ſtone was not deſtroyed by treatment with potaſh. As to the exiſtence of alumina, I do not abſolutely deny it; yet I muſt obſerve, that the whole of the earth which ſeemed to have any reſemblance, however ſmall, to alumina, was at moſt 3 per cent. and there ſeems good reaſon to conſider it as ſilica. Reſpecting the exiſtence of lime in the ſtone of Enſiſheim, I muſt appeal to Profeſſor Barthold, whether, ſuppoſing lime a conſtituent part, ſulphate of lime ſhould not have been formed, as well as ſulphate of magneſia, when ſulphuric acid was generated by igniting the earths and pyrites. And, as to the proportion of alumina, in the ſame ſtone, I would aſk, at leaſt, whether it would have been ſo conſiderable, if the ſolutions formed by acids, after the treatment with potaſh, had been evaporated to the requiſite dryneſs: not to obſerve that no mention is made of any examination of the properties of the earth called alumina. In the proportion of magneſia, I have the ſatisfaction to find my analyſis correſpond very nearly with that of Profeſſor Barthold; and, if what he conſidered alumina were ſuppoſed ſilica, the ſtone preſented to the French Academy, the ſtone of Enſiſheim, and the four I have examined, would agree very nearly in ſiliceous proportions. With reſpect to the nickel, I am confident it would have been found in all, had the metallic particles been ſeparately examined. But, whatever be theſe variations, the mineralogical deſcription of the French Academicians, of Mr. Barthold, and of the Count de Bournon, all exhibit a ſtriking conformity of character, common to each of theſe ſtones; and I doubt not but the ſimilarity of component parts, eſpecially of the malleable alloy, together with the near approach of the conſtituent proportions of the earths contained in each of the four ſtones, the immediate ſubject of this Paper, will eſtabliſh very ſtrong evidence in favour of the aſſertion, that they have fallen on our globe. They have been found at places very remote from each other, and at periods alſo ſufficiently diſtant. The mineralogiſts who have examined them, agree that they have no reſemblance to mineral ſubſtances, properly ſo called; nor have they been deſcribed by mineralogical authors. I would further urge the authenticity of accounts of fallen ſtones, and the ſimilarity of circumſtances attendant on ſuch phenomena; but, to the impartial it would be ſuperfluous, and, to thoſe who diſbelieve whatever they cannot explain, it would be fruitleſs. Attempts to reconcile occurrences of this nature with known principles of philoſophy, it is true, are already abundant; but (as the Earl of Briſtol has well expreſſed) they leave us a choice of difficulties equally perplexing. It is however remarkable, that Dr. Chladni, who ſeems to have indulged in theſe ſpeculations with moſt ſucceſs, ſhould have connected the deſcent of fallen ſtones with meteors; and that, in the narrative of Mr. Williams, the deſcent of the ſtones near Benares, ſhould have been immediately accompanied with a meteor.

The ſtriking conformity of character in theſe ſtones, and their want of reſemblance to all other minerals, join with the hiſtorical teſtimony to prove their origin.

Luminous appearance attending the fall of ſeveral.

No luminous appearance having been perceived during the day on which the ſtone fell in Yorkſhire, it muſt be admitted, rather militates againſt the idea, that theſe ſtones are the ſubſtances which produce or convey the light of a meteor, or that a meteor muſt neceſſarily accompany them *. Yet the ſtone from Sienna fell amidſt what was imagined lightning. but what might in reality have been a meteor. Stones were alſo found after the meteor ſeen in Gaſcony, in July, 1790. And Mr. Falconet, in the memoir I have already quoted, relates, that the ſtone which was adored as the mother of the gods, was a Bœtilia ; and that it fell at the feet of the poet Pindar, enveloped in a ball of fire. He alſo obſerves, that all the Bœtilia had the ſame origin.

Electricity renders theſe ſtones luminous and black.

I ought not perhaps to ſuppreſs, that in endeavouring to form an artificial black coating on the interior ſurface of one of the ſtones from Benares, by ſending over it the electrical charge of about 37 ſquare feet of glaſs, it was obſerved to become luminous, in the dark, for nearly a quarter of an hour; and that the tract of the electrical fluid was rendered black. I by no means wiſh to lay any ſtreſs upon this circumſtance ; for I am well aware, that many ſubſtances become luminous by electricity.

But, ſhould it ever be diſcovered that fallen ſtones are actually the bodies of meteors, it would not appear ſo problematical, that ſuch maſſes as theſe ſtones are ſometimes repreſented, do not penetrate further into the earth : for meteors move more in a horizontal than in a perpendicular direction ; and we are as abſolutely unacquainted with the force which impels the meteor, as with the origin of the fallen ſtone.

Meteor which lately croſſed the county of Suffolk.

Before I cloſe this ſubject, I may be particularly expected to notice the meteor which, a few months ago, traverſed the county of Suffolk. It was ſaid, that part of it fell near Saint Edmundſbury, and even that it ſet fire to a cottage in that vicinity. It appeared, from inquiries made on the ſpot, that ſomething, ſeemingly from the meteor, was, with a degree of reaſon, believed to have fallen in the adjacent meadows ; but the time of the combuſtion of the houſe did not correſpond with the moment of the meteor's tranſition.

Prodigious meteor in America.

A phenomenon much more worthy of attention, has ſince been deſcribed in the Philoſophical Magazine. On the night of the 5th of April, 1800, a body wholly luminous, was ſeen, in America, to move with prodigious velocity. Its apparent

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* In the account of the ſtone which fell in Portugal, no mention is made, either of a meteor or lightning.

ſize was that of a large houſe, 70 feet long; and its elevation above the ſurface of the earth, about 200 yards. The light produced effects little ſhort of ſun-beams; and a conſiderable degree of heat was felt by thoſe who ſaw it, but no electric ſenſation. Immediately after it diſappeared in the north-weſt, a violent ruſhing noiſe was heard, as if the phenomenon were bearing down the foreſt before it; and, in a few ſeconds after, there was a tremendous craſh, cauſing a very ſenſible earthquake. Search being afterwards made in the place where the burning body fell, every vegetable was found burnt, or greatly ſcorched, and a conſiderable portion of the ſurface of the earth broken up. We have to lament, that the authors of this account did not ſearch deeper than the ſurface of the ground. Such an immenſe body, though moving in a horizontal direction, could not but be buried to a conſiderable depth. Should it have been more than the ſemblance of a body of a peculiar nature, the lapſe of ages may perhaps effect what has now been neglected; and its magnitude and ſolitary ſituation become the aſtoniſhment of future philoſophers.

Concerning the immenſe maſs of native iron of S. America.

This leads me to ſpeak of the ſolitary maſs of what has been called native iron, which was diſcovered in South America, and has been deſcribed by Don Rubin de Celis. Its weight was about 15 tons. The ſame author mentions another inſulated maſs of the ſame nature. The whole account is exceedingly intereſting; but, being already publiſhed in the Philoſophical Tranſactions for the year 1788, it needs not be here repeated.

Mr. Prouſt has ſhewn the maſs particularly deſcribed, not to be wholly iron, but a mixture of nickel and iron. The Truſtees of the Britiſh Muſeum, who are in poſſeſſion of ſome fragments of this maſs, ſent to the Royal Society by Don Rubin de Celis, have done me the honour to permit me to examine them; and I have great ſatisfaction in agreeing with a chemiſt ſo juſtly celebrated as Mr. Prouſt.

and that deſcribed by Pallas, and conſidered by the Tartars as fallen from heaven.

The connection which naturally exiſts between one maſs of native iron and another, immediately turns our attention to the native iron in Siberia, deſcribed by Pallas; and this, we are told, the Tartars conſidered as a ſacred relic, which had dropped from heaven. The nickel found in the one maſs, and the traditional hiſtory of the other, not to compare the globular bodies of the ſtone from Benares with the globular concavities and the earthy matter of the Siberian iron, tend to the formation of a chain between fallen ſtones and all kinds of native iron. How far any real affinity exiſts between theſe ſeveral ſubſtances, very obliging friends have afforded me an opportunity to form ſome judgment. I am indebted to Mr. Greville and Mr. Hatchett for portions of almoſt every known native iron: and the Count de Bournon has done me the favour particularly to deſcribe them as follows.

(To be concluded in our next.) Page 148

V.

On the Colours obtained from Metallic Oxides, and fixed by Means of Fuſion on different Vitreous Bodies. By ALEX. BRONGNIART, *Director of the National Manufactory of Porcelain at Sèvres, Engineer of Mines* *, *&c.*

THE art of employing metallic oxides to colour the different vitreous matters has been long known. It is well known that the ancients made coloured glaſs and enamels, and that this art was much practiſed by the Egyptians, who were the firſt that imitated precious ſtones by theſe means.

The colouring of glaſſes by oxides is very ancient.

In modern times the practice of this art has been brought to a high degree of perfection, but its theory has been neglected. It is almoſt the only chemical art to which the new principles of this ſcience have not been applied.

The theory neglected,

In the numerous works which treat of the method of uſing and preparing metallic vitrifiable colours, the authors either give no theory, and conſequently no general principles, or the explanations are founded only on the abſurd hypotheſes which formerly compoſed much of the theory of chemiſtry.

or erroneous.

One of the beſt publications, as it is the work of an enlightened practician, is *The Treatiſe on Painting in Enamel, of Montamy.* The archives of the national manufactory at Sèvres, likewiſe contain ſimple and good proceſſes for the fabrication of colours; Bailly, Fontelliau, and Montigny are the authors; but they are ſimple deſcriptions, without any obſervations that lead to general principles.

Montamy's treatiſe commended.

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* Journal des Mines, No. 67.

Kunckel's work and the Encyclopædias are of little value.

Kunckel's work, the manuſcripts of Hellot, in the poſſeſſion of the manufactory at Sévres, and the two Encyclopædias, preſent only an undigeſted collection, and a multitude of proceſſes collected together from all quarters, without choice or reaſon. With a ſlight knowledge of the art it is eaſier to invent a new proceſs of fabrication, than to diſcover which among this multitude of receipts we ought to prefer.

Science is conſiderably advanced when the facts compoſe a body of doctrine.

It has been remarked, that one of the moſt certain criterions of the progreſs made to the perfection of a ſcience, is the poſſibility it leaves of a re-union of the facts that compoſe it, in a body of doctrine from which general principles may be deduced. At the preſent period only does it deſerve the name of a ſcience, and it is to the expoſition of theſe principles, that the ſtriking, though inaccurate name of the philoſophy of ſcience has been given.

The arts would be greatly advanced by ſimilar treatment.

The arts, which oftener compoſe a branch of ſcience, than the ſimple application of one of its parts, preſent facts equally capable of being united in a body of doctrine; they will attain this valuable degree of perfection if practiſed by men accuſtomed to trace the connections, and deduce the conſequences of the events that paſs under their obſervation.

Philoſophers might acquire them.

Philoſophers, whoſe more elevated ſpeculations ſeclude them from the practice of theſe arts, would then perceive their principles with greater eaſe; they could more directly apply their reſearches to the progreſs of the art, which being then directed by reaſon, would become more certain, direct and rapid in its advancement.

The preſent memoir aims to make this diſpoſition as to the art of vitrifiable colours.

Being convinced that the art of preparing and uſing vitrifiable colours is ſuſceptible of receiving the application of the means of perfection; and that the facts of which it is compoſed are ſufficiently numerous and accurate to be preſented in a general outline, I have concluded that the preciſe knowledge of theſe facts, and of the principles which unite them, which naturally lead to an explanation of a number of the reſults, might be intereſting to chemiſts, who, being occupied by more general and important reſearches, cannot attend to all the details of a complicated art.

I wiſhed likewiſe to give an accurate account of the principles of this art to chemiſts, that they might determine with certainty on the new operations to which the proceſſes ſubmitted to their judgment might lead.

Laſtly,

Exhibition of the ſecret of the colours at Sévres.

Laſtly, I thought it would be advantageous to the progreſs of the art, and that it was incumbent on the national manufactory at Sévres to diſcloſe the pretended ſecret of the compoſition of colours for porcelain which do not change in the fire. Theſe colours were preſented to the Inſtitute in the year 6, by a manufacturer of porcelain, juſtly admired for the beauty of the works produced at his manufactory. I ſhould not have publiſhed this ſecret had it been given to me in confidence, but as it is known, I hope I ſhall no longer be ſuſpected of the leaſt breach of integrity.

From what has been ſaid, it will be ſeen that my object is not to give the exact compoſition of all the vitrifiable colours at full length. Such a work could not be the ſubject of a ſimple memoir.

Vitrifiable colours are made by met. oxides.

It is known that metallic oxides are the baſes of all vitrifiable colours; but ſome of the metallic oxides are not proper for this uſe; and as they are not vitrifiable without ſome admixture, they can ſeldom be employed alone.

Not thoſe which are volatile, or eaſily change their oxigenation.

The very volatile oxides, and thoſe that do not adhere ſtrongly to the oxigen they contain in abundance, either cannot be employed in any manner, as is the caſe with the oxides of mercury and arſenic, or elſe they can be employed only as agents. The colour they afford cannot be depended on, becauſe they loſe it, in proportion as they loſe their oxigen by the ſlighteſt heat. Such are the puce and red coloured oxides of lead, the yellow oxide of gold, &c.

Thoſe oxides, in which the proportion of oxigen is capable of being varied too eaſily, are ſeldom employed. The black oxide of iron is never employed for that colour. The green oxide of copper is in many circumſtances very uncertain in its effect.

Oxides cannot be uſed alone;

I have remarked that the oxides do not melt alone; though when placed in thin layers on vitrifiable ſubſtances they can be made to adhere by means of a violent fire; but, with the exception of the oxides of lead and biſmuth, they afford only dull colours. The very violent heat which is frequently required to fix them, changes or totally deſtroys their colours. Some flux is therefore added to all the metallic oxides.

but require a flux. Lead and ſilex, or lead, ſilex, and borax.

This flux is either glaſs of lead and ſilex, or glaſs of borax, or a mixture of both.

The flux fixes the colour at a moderate heat; leſs than would deſtroy the colour.

Its general effect is to give brilliancy to the colours after their fuſion, to fix them upon the piece that is painted by ſoftening its ſurface, to envelope the metallic oxides, to preſerve their colour by defending them from the contact of the air, and particularly to facilitate the fuſion of the colour at a ſlightly elevated temperature, not capable of deſtroying it.

The obſervation which proves this latter uſe of ſolvents is taken from delicate colours, ſuch as the carmines produced from gold; theſe colours require a much greater proportion of flux than the others.

Very tranſient colours are mixed with the flux without previous fuſion.

Sometimes the oxides are directly employed, and ſimply mixed with their ſolvents, without having been previouſly melted with it; ſuch are the colours which the violence or repetition of heat would alter. It is eaſy to conceive that a ſtronger and more laſting heat is required to fuſe a crucible of coloured glaſs than a layer of colour, which is not the tenth of a millimetre in thickneſs.

I ſhall return to this ſubject when treating of the red colours obtained from gold.

Others are fuſed with it and then ground.

In many caſes the oxides are previouſly fuſed with their ſolvents, and afterwards ground. When I ſpeak of colours in particular, I ſhall mention thoſe which are ſubjected to this fuſion.

Theſe general principles are too ſimple to require further explanation.

This treatiſe is confined to vitrifiable colours on glazes or glaſs.

I ſhall only ſpeak of the application of metallic colours to vitreous bodies or ſurfaces.

Theſe grounds are three.

Theſe bodies may be divided into three very diſtinct claſſes, from the nature of the ſubſtances that compoſe them, the effects produced on them by the colours, and the changes they undergo. Theſe are,

1. Enamel.

1ſt. Enamel; ſoft porcelain, and all the glazes, enamels, or glaſſes, which contain lead in any conſiderable quantity.

2. Hard porcelain.

2d. Hard porcelain, or ſuch as is glazed with feldſpar.

3. Common glaſs.

3d. Glaſs in which there is no lead, ſuch as the common window glaſs.

I ſhall ſucceſſively examine the principles of compoſition of theſe colours, and the general phenomena they preſent on theſe three grounds or ſupporters.

Colours for enamel painting have been longeſt known: the receipts in the works I have mentioned at the beginning of this eſſay all relate to theſe colours.

Enamel colours have been long known.

It is known that enamel is a glaſs rendered opake by oxide of tin, and very fuſible by the oxide of lead. It is this laſt which in particular gives it properties very different from thoſe of the other excipients of metallic colours. Hence all the glaſſes and glazes that contain lead, have the properties of enamel, and what we may aſſert of the one, will apply to the other with very little difference.

Enamel is glaſs rendered fuſible by ox. lead, and opake by ox. tin.

Such are the white and tranſparent glazes of Dutch or Delft ware, and the glaze of the porcelain called ſoft *(tendre.)*

It is the glaze of Delft ware, &c.

This porcelain, the firſt made in France, particularly at Sévres, and indeed for a long time almoſt excluſively at this manufactory, has for its baſe a vitreous frit nearly opake, capable of being acted upon by marl, and its glaze is a very tranſparent glaſs containing much lead.

Soft porcelain of Sévres.

The colours made uſe of are the ſame as thoſe for enamelling, conſequently the changes theſe colours undergo in enamel, muſt take place in this ſpecies of porcelain, becauſe, as we ſhall ſoon ſee, the cauſes of the change are the ſame in both.

Colours the ſame as for enamelling.

The colours for enamel and tender porcelain require leſs flux than the others, becauſe the glaſs on which they are placed, ſoftens ſufficiently to be penetrated by them.

Theſe require leſs flux becauſe the glaze is very fuſible.

This ſolvent may be either the mixture of glaſs of lead, and pure ſilex called *rocaille*, or this ſame glaſs mixed with that of borax.

The flux before mentioned.

Montamy ſays that glaſs of lead ought not to be uſed in the flux for enamel; he employs borax alone. He then dilutes or makes up his colours in a volatile oil.

On the contrary, the painters of the manufactory at Sévres, uſe only colours without borax, becauſe they dilute them with gum, and borax does not dilute well this way. I am convinced that both methods are equally good, and that Montamy is not juſtified in excluding the fluxes of lead, as they are employed without inconvenience every day, and even render the management of colours more eaſy.

Glaſs of lead is a good material.

I have remarked, that in the baking of theſe colours the glaze is ſoftened ſo much as to be eaſily penetrated by them; this is one great cauſe of the change they undergo. They be-

The ſoft glaze very much dilutes the colours.

come diluted by the mixture with the glaze, and the firſt fire changes a painting apparently finiſhed, into a very ſlight ſketch.

And the oxide of lead acts chemically on them.

The oxide of lead contained in the glaze is a more powerful cauſe of the great changes theſe colours undergo. Its deſtructive action is principally exerciſed on the reds of iron, and is very remarkable. I ſhall relate ſome experiments that particularly prove this.

Theſe colours themſelves are ſcarcely changeable, except from the ground they are placed upon.

It has already been ſhewn that the two principal cauſes of the change which colours on enamel and tender porcelain undergo, do not relate to the compoſition of theſe colours, but intirely to the nature of the glaſs upon which they are placed. The aſſertion that the colours of porcelain are ſubject to conſiderable change, relates to the colours of ſoft porcelain, *(porcelain tendre)* a ſpecies of ware now almoſt totally abandoned.

The paintings before mentioned require much touching up.

It follows from what I have ſaid, that the paintings of porcelain require to be ſeveral times re-touched and burned, in order to poſſeſs the neceſſary ſtrength. Though theſe paintings have always a certain ſoftneſs, they are conſtantly more brilliant, and never ſubject to the inconvenience of ſcaling off.

Hard porcelain; with glaze of feld ſpar,

The hard porcelain, according to the diviſion I have laid down, is the ſecond ſpecies of ground or excipient for the metallic colours. It is known that the baſe of this porcelain is a very white argil, called *kaolin*, mixed with a ſiliceous and calcareous ſolvent, and the glaze of which is nothing but feld ſpar fuſed without an atom of lead.

or of Saxony, receives two kinds of colour; viz. on the glaze or painting colours,

This porcelain, which is that of Saxony, is of much later date at Sévres than the ſoft or tender. The colours employed are of two kinds, the firſt uſed for repreſenting different objects, are baked with a very inferior fire to that required for the baking of the porcelain itſelf. They are very numerous and varied.

or beneath it; or ground colours.

The others, which require to be fuſed at as great a heat as that for baking the porcelain, are laid on the general ſurface. They are much leſs numerous.

The colours for painting are nearly the ſame as for the ſoft porcelain; but contain more flux.

The colours for painting are made up very nearly of the ſame materials as thoſe for tender porcelain; they only contain more flux. This flux is compoſed of the glaſs of lead, (called *rocaille*) and of borax. I have not yet met with any work that treats of the compoſition, uſe and effects of theſe colours. In fact, it has no where been aſſerted in print, that all theſe colours except one are unchangeable in the fire; whereas it has been often aſſerted in books, that paintings in enamel are ſubject to conſiderable change.

The flux is glaſs of lead and borax.

When the porcelain is put in the fire to bake the colours, the feld ſpar glaze dilates and opens its pores, but does not become ſoft. As the colours do not penetrate it, they are not ſubject to the changes they undergo on tender porcelain. It muſt however be obſerved, that they loſe a little of their intenſity, by acquiring the tranſparence given them by the fuſion.

The hard glaze does not run nor dilute the colours,

When works of little importance are made they need not be re-touched; but this is neceſſary when a painting is to be highly finiſhed. This re-touching is not diſtinguiſhable in the paintings on porcelain from that of any other ſpecies of painting.

ſo that only fine work needs retouching.

One of the great inconveniences of theſe colours, is the facility with which they ſcale or fly off when the fire is often applied.

The colours are apt to ſcale.

This has been particularly remarked at Sévres, on account of the ſolidity and infuſibility with which porcelain is there manufactured. But theſe qualities cauſe it to reſiſt the alternations of heat and cold for a longer time, and gives its ground a more brilliant white colour. On the other hand, the porcelains of Paris being more vitreous, tranſparent, and of a blueiſh caſt, generally crack if boiling water is frequently poured into them.

Hard porcelain is more beautiful, and ſtands changes of heat and cold better than the ſoft.

In order to remedy this evil without altering the quality of the body, I ſoftened the glaze a little, by introducing more ſiliceous or calcareous flux according to the nature of the feld ſpar. This method ſucceeded, and for this twelvemonth paſt the colours have paſſed two and three times through the fire without cracking, provided there be not too much flux, and they be not laid on too thick.

The ſcaling is prevented by ſoftening the glaze.

It has been remarked, that when ſoda and potaſh were introduced the colours ſcaled; ſo that they cannot be uſed as fluxes. Theſe alkalies being volatilized abandon the colours, which cannot adhere to the glaze by themſelves.

Alkalies are too volatile to be uſed in this art.

I have obſerved that other colours are likewiſe prepared, which being laid upon the general ſurface, are fuſed by the ſame fire as bakes the porcelain. Theſe colours are but few; becauſe there are few metallic oxides that can ſupport ſuch a

Colours to be laid on and baked by the firſt fire are few.

fire without being volatilized or diſcoloured. Their ſolvent is the feld ſpar. As they incorporate with the glaze they never crack, and are more brilliant.

The third receptacle of metallic vitrifiable colours is glaſs without lead.

Art of painting on glaſs,

is not loſt.

The application of theſe colours conſtitutes the art of painting on glaſs; an art much practiſed in former ages, but which is ſuppoſed to be loſt, becauſe out of faſhion. It however too immediately depends on the art of painting on enamel and porcelain to be intirely loſt. Deſcriptions of the proceſſes may alſo be found in many different works.

Books in which it is treated. Original.

Leviel's work is only a compilation.

A book intitled, *L'Origine de l'Art de la Peinture ſur Verre,* publiſhed at Paris in the year 1693, and *Le Traité de l'Art de la Verrerie,* by *Neri* and *Kunckel,* ſeem to be the firſt works containing complete deſcriptions of this art. Thoſe publiſhed ſince, even the great work of *Leviel,* which conſtitutes part of *Les Arts et Metiers* of the academy, and of the *Encyclopédie Methodique,* are only compilations from the two former works.

The others afford very looſe inſtruction.

It is ſomewhat remarkable, that if we follow the proceſſes exactly as they are deſcribed in theſe works, as I have done with ſome of them, the colours of which they pretend to give the receipt would never be fabricated. They only ſerve to ſhew an able practician the method, and leave it to him to correct or make additions. This was found to be the caſe by Citizen Meraud, who was engaged to prepare them for the manufactory of Sévres. He was obliged to make the colours for painting on glaſs rather from his own experience, than from the inſtructions in the works I have juſt mentioned.

The colours for glaſs are nearly the ſame as for enamel; but ſome of them are dull for want of the opake ground.

The limits of a memoir will not permit me to enter into hiſtorical details on the art of painting on glaſs; its hiſtory is given at length in Leviel's work; the materials and fluxes which enter into the compoſition of the colours for painting on glaſs, are in general the ſame as thoſe applied to porcelain. They vary only in their proportions; but a great number of the colours uſed for enamel and porcelain cannot be applied to glaſs; many of them when ſeen by tranſmitted light intirely change their aſpect, and exhibit an obſcure tint which can be of no uſe when deprived of the white ground which throws them out. We ſhall point out theſe when we treat of the colours in particular. Thoſe colours which can be uſed on this body, ſometimes change in the baking, and acquire a great tranſparence. They are generally beautiful only when placed between the eye and the light, and then they anſwer the purpoſe intended in painting on glaſs.

There is more difficulty in baking plates of coloured glaſs than is commonly thought. The bending of the piece, and alteration of the colours, are to be avoided. All the treatiſes we have conſulted recommend the uſe of gypſum. This method ſometimes ſucceeded, but generally the glaſs became white, and cracked in all directions. It appears that the glaſſes that are too alkaline, and which are far the moſt common in clear white glaſſes, are attacked by the hot ſulphuric acid of the ſulphate of lime. We were able with eaſe to bake much larger glaſſes than any before painted, by placing them on very ſmooth plates of earth or unglazed porcelain.

Difficulties of baking glaſs plates.

On the uſual ſupport of gypſum, the alkali of the glaſs is attacked, and the piece broken.

Porcelain biſcuit is a much better ſupport.

Concerning the ſeveral particular Colours.

After having collected the general phenomena which each claſs of vitrifiable colours offers with regard to the bodies on which they are placed, I muſt ſhew the particular and moſt intereſting phenomena which every principal ſpecies of colours employed on tender porcelain, on glaſs, and in the fire that bakes the porcelain, preſent.

Particular colours.

Concerning the Reds, Purples, and Violets obtained from Gold.

The carmine red is obtained from the purple precipitate of Caſſius. It is mixed with about ſix parts of its flux, and this mixture is directly employed without being firſt fuſed. It is then of a dirty violet, but acquires the beautiful carmine by baking. It is however very delicate; a little too much heat or carbonated vapours eaſily ſpoil it; yet it is more beautiful when baked with charcoal than with wood.

Carmine red from gold,

is very periſhable in the fire,

This colour and the purple which differs little from it, as well as all the ſhades which are obtained from their mixture with other colours, really change in all porcelains and in the hands of all operators. But this is the only one that changes on hard porcelain. It may be replaced by a ſubſtitution of roſe colour from iron, which does not change; ſo that by excluding from the pallet the carmine made from gold, and ſubſtituting the

as well as its ſhades.

It is the only colour that changes on hard porcelain;

and may be changed for oxide of iron in this uſe, though this oxide fails *on enamel.*

roſe-coloured oxide of iron here ſpoken of, we have a pallet compoſed of colours none of which are ſubject to any remarkable change. This roſe-coloured oxide of iron has been long known, but was not employed *on enamel,* becauſe it is then ſubject to conſiderable change. Or perhaps when the painters on enamel became painters on porcelain, they continued to work according to their ancient method.

We might ſuppoſe that by previouſly reducing the colour named carmine, already mixed with its ſolvent, into a vitreous matter, the laſt tint would be obtained. But as I have already proved, the fire which muſt be uſed to melt this vitreous maſs deſtroys the red colour. Beſides, it is found that, to obtain this colour in perfection, it is neceſſary to paſs it through the fire as little as poſſible.

Fabrication of the gold carmine.

The carmine of tender porcelain is made with fulminating gold gently decompoſed, and muriate of ſilver; there is no tin in it, which proves that it is not neceſſary for the fabrication of a purple colour, that the oxide of this laſt metal and that of gold ſhould be combined.

Violet from gold ſtands better on ſoft porcelain.

Violet is likewiſe obtained from the purple oxide of gold. This colour proceeds from having a greater quantity of lead in the flux, and it is nearly of the ſame tint whether crude or baked.

But they all fail on hard.

Theſe three colours totally diſappear in the ſtrong fire proper to bake porcelain.

The carmine and purple are not good upon glaſs; but the violet is beautiful.

Carmine and purple afforded us upon glaſs, only tints of a dirty violet. The violet, on the contrary, has a very beautiful effect; but it is ſubject to change to a blue. I cannot yet aſcertain the cauſe of ſo ſingular a change, as I obſerved it for the firſt time only a few days ago.

(To be concluded.)

VI. *Examination*

VI.

Examination of the ſuppoſed igneous Origin of the Rocks of the Trapp-formation. By Mr. Robert Jameson. *From the Author.*

Sheriff Brae, Leith, Sept. 8, 1802.

Aqueous origin of the rocks of trapp-formation.

That the Rocks of the Trapp-formation have been depoſited from a State of Solution and Suſpenſion in Water, is a Truth, which has been demonſtrated by the Wernerian Geognoſie.

It is not at preſent my intention to ſtate the proofs of this opinion: the ſubject of the following obſervations is the examination of Mr. Playfair's account of theſe rocks.

Prof. Playfair's ſtatement: that the columnar form of baſalt is the effect of fire, becauſe it is ſeen in lavas.

At ſection 64 of his illuſtrations of the Huttonian theory, he obſerves, "The columnar ſtructure ſometimes found in that ſpecies of whinſtone called baſaltes, is a fact which has given riſe to much diſcuſſion; and it muſt be confeſſed, that though one of the moſt ſtriking and peculiar characters of this foſſil, is not that which gives the cleareſt and directeſt information concerning its origin. One circumſtance, however, very much in favour that baſaltic rocks owe their origin to fire, is, that the columnar form is ſometimes aſſumed by lava actually erupted from volcanos. Now, it is certainly of no ſmall importance to have the ſynthetic argument on our ſide, and to know that baſaltic columns can be produced by fire; though no doubt, to give abſolute certainty to our concluſion, it would be neceſſary to ſhew, that there are in nature no other means but this by which theſe columns can be formed. This ſort of evidence can hardly be looked for; but ſince the power of fuſion to produce the phenomena in queſtion is perfectly eſtabliſhed, and ſince the production of the ſame phenomena in the humid way is a mere hypotheſis, if there be the leaſt reaſon to ſuſpect the action of ſubterraneous heat* as one of the cauſes of mineralization,

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* The phenomena of volcanos and hot ſprings are conſidered by Profeſſor Playfair as proofs of a ſubterranean heat, unconnected with the decompoſition of any mineral ſubſtance. The Wernerian geognoſie, however, has ſhewn us, that volcanos and hot ſprings originate in the neweſt flotz trapp-formation, therefore we have ſufficient

Reply. That the deſcriptions of lavas by Spallanzani and others, are incorrect as to the appearance and the matter itſelf; and that the aqueous origin of baſalt is deducible from ſimilarity of ſtructure in other foſſils ſo formed.

ralization, every maxim of ſound philoſophy requires that the baſaltic ſtructure, in all caſes, ſhould be aſcribed to it." The accounts given by Spallanzani and other travellers of columnar lava, are moſt evidently incorrect; the appearances they have deſcribed are accidental rents, ſuch as we obſerve in ſandſtone, limeſtone, and even in baſalt. Many of the priſmatic lavas deſcribed by Spallanzani, are either baſalt, greenſtone, or porphyry. That baſalt columns have been formed by chryſtallization from a ſtate of ſolution in water, is rendered probable from the analogy of their ſtructure with that of other foſſils which have been certainly ſo formed. Baſalt we know occurs in the ſame maſs in tabular and columnar diſtinct concretions, correſponding in this particular with the lamellar and columnar diſtinct concretions of truly chryſtalline foſſils *.

P. The preſence of pyrites urged as the proof of igneous production;

At ſection 66 we are informed, "That a mark of fuſion, or at leaſt of the operation of heat, which whinſtone poſſeſſes in common with many other foſſils, is its being penetrated with pyrites, a ſubſtance, as has been already remarked, that is of all others moſt excluſively the production of fire."

but theſe are very abundant among matters never ſubjected to fire.

Mr. Playfair probably did not know that the great beds of bituminated wood which are found covered with ſand, clay, &c. in the alluvial hills, are very frequently much impregnated with pyrites; and that lately Mr. Hatchett had alſo demonſtrated its formation in the humid way.

P. Inſulated maſſes of indurated ſandſtone in whin, urged as a proof of igneous fuſion.

He continues, "Another mark of fuſion, more diſtinctive of whin, is, that both in veins and maſſes it ſometimes includes pieces of ſandſtone, or of the other contiguous ſtrata, completely inſulated, and having the appearance of fragments of rock floating in a fluid ſufficiently denſe and ponderous to ſuſtain their weight. Though theſe fragments have been too refractory to be reduced into fuſion themſelves, they have not remained intirely unchanged; but are, in general, extremely indurated, in compariſon of the rock from which they appear

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cient proof of the exiſtence of quantities of inflammable matter, equal to the ſupport of all the volcanoes that have, or can exiſt on our globe.

* When baſalt, having a columnar ſtructure, occurs in veins, we find that theſe columns are perpendicular to the walls of the vein. This phenomena I have frequently obſerved, and conſider it as ſupporting in ſome degree the opinion mentioned above.

to

It is denied that theſe are inſulated or indurated.

to have been detached." That the pieces of ſandſtone are not inſulated, I have proved in a former paper. The occurrence of maſſes of ſandſtone in veins of baſalt, grunſtone, and wacken, is well known; but that there are maſſes more indurated than the other ſandſtone of this formation, muſt be denied, as we know that ſandſtone beds occur in this formation of all degrees of induration.

P. Extraordinary induration of the ſtrata in contact with whinſtone;

Section 67. "Similar inſtances of extraordinary induration are obſerved in the parts of the ſtrata in contact with whinſtone, whether they form the ſides of the veins, or the floors and roofs of the maſſes into which the whinſtone is diſtributed. The ſtrata, whether ſandy or argillaceous, in ſuch ſituations are uſually extremely hard and conſolidated; the former in particular loſe their granulated texture, and are ſometimes converted into perfect jaſper. This intereſting remark was firſt made by Dr. Hutton, and the truth of it has been verified by a great number of ſubſequent obſervations."

accounted for by the depoſition of baſaltic ſolution in the pores of ſofter ſtrata.

The flinty ſandſtone which is characteriſtic for the neweſt flötz trapp-formation, is uſually covered by clay, wacken, baſalt, or grunſtone, in the order here mentioned: ſometimes the clay is wanting, when the ſandſtone is covered by wacken, or when the wacken has been carried away, or not depoſited, by baſalt or grunſtone. We have thus, to uſe the Huttonian language, the ſandſtone equally indurated under a bed of clay as under one of baſalt or grunſtone: it is therefore plain, that no argument can be drawn from the ſituation and nature of this ſandſtone in favour of the igneous ſyſtem. When the walls of veins are indurated, we can eaſily trace it to the depoſition of a portion of the baſaltic ſolution in the pores of the ſofter ſtrata.

P. The appearance of charring or coaking of coal in the vicinity of veins of whin, adduced as a proof of their former ſtate of fuſion.

Section 68. "To the ſame excellent geologiſt (Dr. Hutton) we are indebted for the knowledge of an analogous fact attendant on the paſſage of whinſtone veins through coal ſtrata. As the beds of ſtone where they are in contact with the veins of whin, ſeem to acquire additional induration, ſo thoſe of coal, in like circumſtances, are frequently found to have loſt their fuſibility, and to be reduced nearly to the ſtate of coke or charcoal. The exiſtence of coal of this kind has been already mentioned, and has been conſidered as a proof of the operation of ſubterraneous heat. In the inſtances here referred to, that is, where the charring of the coal is limited to thoſe parts of the ſtrata which are in contact with the whin, or in its immediate vicinity, the heat is pointed out as reſiding in the vein; and this is to be accounted for only on the ſuppoſition of the melted whin, at a period ſubſequent to the conſolidation of the coal, having flowed through the openings of the ſtrata. The heat has been powerful enough in many places to drive off the bituminous matter of the coal, and to force it into the colder and more diſtant parts. Few facts in the hiſtory of foſſils are more remarkable than this, and none more directly aſſimilates the operations of the mineral regions with thoſe that take place at the ſurface of the earth."

In reply. This effect is aſcribed to the percolation of water.

An eminent Iriſh geognoſt is of opinion, that the appearances deſcribed by Profeſſor Playfair may have been produced by the percolation of water along the ſides of the baſalt vein. This explanation will be ſatisfactory to thoſe who have witneſſed the effects produced by the weather on coal. This altered coal, however, muſt not be confounded with the glanz and columnar coal, becauſe theſe differ from it in oryctognoſtical and geognoſtical characters.

The argument contained in ſection 69, is refuted by what Werner mentions in his admirable book on the formation of veins.

P. That the whinſtone was introduced between the ſtrata, is deducible from the ſimilarity of the parts ſo ſeparated.

Section 71. "Again, if it be true that the maſſes of whinſtone, thus interpoſed among the ſtrata, were introduced there after the formation of the latter, we might expect to find, at leaſt in many inſtances, that the beds on which the whinſtone reſts, and thoſe by which it is covered, are exactly alike. If theſe beds were once contiguous, and have been only heaved up and ſeparated by the irruption of a fluid maſs of ſubterranean lava, their identity ſhould ſtill be recogniſed. Now this is preciſely what is obſerved; it is known to hold in a vaſt number of inſtances, and it is ſtrikingly exemplified in the rock of Saliſbury Crag near Edinburgh." If this argument was correct, it ſhould follow, that the beds of foliated limeſtone containing ſhells, which are found between ſtrata of ſandſtone, had been ejected from below. This fact cannot be evaded by ſaying, that the limeſtone has been only ſoftened; it is plain if the foliated fracture (according to the Huttonian ſyſtem) is a character of fuſion in one ſtone, ſo muſt it in all others.

This argument affirmed to prove too much, becauſe it would apply to other middle ſtrata.

P. The nodules of agate in whinſtone appear to have been fluid;

At ſection 73 we have the following obſervations on the formation of agates: "Some of the ſpecies of whinſtone are the common matrices of agates and chalcedonies, which lie incloſed in them in the form of round nodules. The original fluidity of theſe nodules is evinced by their figured and ſometimes chryſtallized ſtructure, and indeed is ſo generally admitted, that the only queſtion reſpecting them is, whether this fluidity was the effect of heat or ſolution. To anſwer this queſtion Dr. Hutton obſerves, that the formation of the concentric coats of which the agate is uſually compoſed, has evidently proceeded from the circumference toward the center, the exterior coats always impreſſing the interior, but never the reverſe. The ſame thing alſo follows from this other fact, that where there is any vacuity within the agate, it is uſually at the center, and there too are found the regular chryſtals, when any ſuch have been formed. It therefore appears certain, that the progreſs of conſolidation has been from the circumference inwards, and that the outward coats of the agate were the firſt to acquire ſolidity and hardneſs. Now it muſt be conſidered that theſe coats are highly conſolidated; that they are of very pure ſiliceous matter, and are utterly impervious to every ſubſtance which we know of, excepting light and heat. It is plain therefore, that whatever at any time, during the proceſs of conſolidation, was contained within the coats already formed, muſt have remained there as long as the agate was intire, without the leaſt poſſibility of eſcape. But nothing is found within the coats of the agate ſave its own ſubſtance; therefore no extraneous ſubſtance, that is to ſay no ſolvent, was ever included within them. The fluidity of the agate was therefore ſimple, and unaſſiſted by any menſtruum. In this argument nothing appears to me wanting that is neceſſary to the perfection of a phyſical, I had almoſt ſaid of a mathematical demonſtration. It ſeems, indeed, to be impoſſible that the igneous origin of foſſils could be recorded in plainer language, than the phenomenon which has been juſt deſcribed."

and to have conſolidated from without:

They are pure ſiliceous matter, impervious to any other ſubſtance;

whence this fluidity is inferred to have been ſimple, without any menſtruum.

It is well known to geognoſts, that during the depoſition of certain kinds of ſtrata, conſiderable quantities of air have been formed, and this in endeavouring to eſcape has given them a cellular ſtructure. The almond-ſtone (mandelſtein) in which agates are moſt frequently found, has acquired its cellular ſtructure in this manner. Theſe cells appear to have been ſucceſſively filled with a ſtony ſolution, out of which thin coats of jaſper, carnelian, chalcedony, &c. have been precipitated, according as the precipitate altered or was finer. We are naturally

Reply. The cellular ſtructure of the ſtrata aſcribed to gas;

the cells are ſtated to have been ſucceſſively filled with a ſtony ſolution;

turally led to the idea of an increaſing fineneſs in the ſolution, becauſe we obſerve moſt diſtinctly that the outer coat is compoſed of the coarſeſt precipitate, and that as it approaches the middle, it is always finer and finer; ſo that at laſt, out of the fineſt ſolution, amethyſt ſhoots, and, when there is room enough, chryſtallizes. That the ſolution out of which the precipitation took place was truly chemical, is demonſtrated not only by the character of the ſubſtances, but alſo by the diſpoſition of the coats; for we can obſerve that they have followed the attraction of the walls of the cell, and are ſo accurately depoſited upon it, that an inequality, or elevation upon the outer wall, continues to be obſerved through all the ſuperincumbent coats to the center of the agate. That the ſolution had come from without, and by infiltration penetrated into the cells, is moſt diſtinctly to be ſeen in ſpecimens of agate when they are properly cut. If the cutting has been judiciouſly done, we can always obſerve the opening by which the ſolution has entered. This elegant explanation is an outline of that delivered by the illuſtrious Werner in his geognoſtical lectures, and is a demonſtration of the aqueous origin of agate.

whence the precipitation of nodules took place.

The opening of infiltration obſervable.

Flinty foſſils not impervious to water.

That flinty foſſils, as ſtated by Mr. Playfair, are impervious to water, has never been proved; on the contrary, it is well known to collectors of foſſils, that if the cellular rock chryſtal, which contains water, is kept in a cabinet for ſome years, the water gradually diſappears.

Other obſ.

Sections 249, 250, 1, 2, are occupied with an unſucceſsful attempt to reconcile the appearances on the hill of Scheibenberg with the Huttonian theory. Without inſiſting upon the Wernerian doctrine of formations, which gives the death-blow to all partial explanations like thoſe propoſed by the Huttonian theory, I ſhall merely mention a few facts reſpecting the individual relation of theſe rocks, to ſhew the inſufficiency of the explanation attempted by Profeſſor Playfair.

Situations of wacken admitted by Prof. P.

1. Sandſtone is ſometimes obſerved covering wacken.

2. Gravel, covered by ſlaty clay and baſalt, has been obſerved lying on wacken.

3. Veins of wacken ſometimes traverſe gneiſs, porphyries, ſandſtone, &c.

adduced to prove that it was not hardened by fire.

Now, if wacken is clay hardened by the ſuper-poſition of melted baſalt, we muſt ſuppoſe the ſame effect to have been produced by the ſlaty clay, ſandſtone, or gravel. It is equally impoſſible to explain wacken veins by this hypotheſis of Profeſſor Playfair.

From ſection 255 to 259 is principally occupied in endeavouring to ſhew, that from the wedge-ſhape irregularities in thickneſs, and the frequent great inclination of beds of baſalt, they would not have been depoſited from a ſtate of ſolution in water.

Wedge-ſhape irregularities of baſalt,

To explain the appearances deſcribed in theſe ſections will be no difficulty to the Neptunian, they are what he daily meets with, and are illuſtrative of the depoſition of theſe ſtrata from water. Let us conceive a bed of baſalt depoſited upon an unequal ſurface, that the water breaks it (either before it has become ſolid, or afterwards) into ſhapes reſembling thoſe deſcribed by Profeſſor Playfair, afterwards that ſandſtone is depoſited upon this broken bed; and we have a complete picture and explanation of the wedge-ſhape, irregularities in the thickneſs, and every variety of inclination of the ſtrata of baſalt. This is illuſtrated by figure 1 and 2, Plate VIII.

accounted for by aqueous depoſition on an unequal ſurface; broken by water; and ſandſtone depoſited thereon.

A very ſtrong objection has been made to the igneous origin of baſalt and grunſtone, which has always appeared to me completely unanſwerable: it is this: If theſe rocks had been formerly in a ſtate of complete fuſion, they ſhould either appear as glaſs, or as a ſimple maſs, not containing any ſubſtance of a different nature from the maſs itſelf.

Objection to the igneous formation of baſalt, that it is not ſimple, but contains other bodies.

Mr. Playfair, at the concluſion of his obſervations on the trapp-formation rocks, remarks, "Notwithſtanding all this accumulated and unanſwerable evidence for the igneous formation of baſaltes, *(the evidence I have examined in the preceding pages)* a great objection would ſtill remain to our theory, were it not for the very accurate and concluſive experiments concerning the fuſion of this foſſil, made by Sir James Hall. A ſtrong prejudice againſt the production of any thing like a real ſtone by means of fuſion, had ariſen even among thoſe mineralogiſts who were every day witneſſes of the ſtony appearance aſſumed by volcanic lava. They ſtill maintained, on the authority of their own imperfect experiments, that nothing but glaſs can ever be obtained by the melting of earths and ſtones, in whatever way they are combined.

P. Sir James Hall's experiments adduced in proof, that the ſtony character is producible by fuſion.

An ingenious naturaliſt, after deſcribing a block of baſaltes, in which he diſcovered ſuch appearances as inclined him to admit its igneous conſolidation, rejects that hypotheſis, merely from the imaginary inability of fire to give to any ſubſtance a ſtony character: " Quelque mélange," ſays he, " de terres que l'on ſuppoſe, quelque ſoit le tems que l'on emploie, il eſt trés certain que l'on n'obtiendra pas, par la ſeul fluide igné, ni baſalte, ni rien que lui reſemble."

Sir James Hall's experiments have completely demonſtrated the contrary of what is here aſſerted; they have added much to the evidence of the Huttonian ſyſtem; and independently of all theory, have narrowed the circle of prejudice and error."

It is anſwered, that Sir James Hall's chryſtallite and the original ſtone are different.

Sir James Hall, however, has only ſhown, that when baſalt, grunſtone, or wacken, are completely melted and then ſlowly cooled, the glaſſy character diſappears, and the maſs aſſumes that of indiſtinct chryſtallization. This chryſtallized maſs he denominates chriſtalite, as bearing a moſt ſtriking reſemblance to the natural ſtones. To the oryctognoſt, however, the glaſs and the indiſtinctly chryſtallized maſs are equally different from the original ſtone, therefore the arguments drawn from theſe experiments, in favour of the igneous origin of the rocks of the trapp-formation, are unfounded, and the objection which has been now ſtated remains in full force.

Objections to Sir James Hall's opinion, that lava has been in a ſtate of complete fuſion, &c.

Sir James Hall's opinion, that lava has been in a ſtate of complete fuſion, and by ſlow cooling has acquired its preſent character, is probably incorrect. The well known facts, that lava has run into the ſea, rivers, and lakes, without aſſuming the glaſſy character; that iſlands compoſed of cellular lava have been formed in the ſea; and laſtly, the low degree of heat poſſeſſed by lava ſtreams, ſhew, that it has not been in a ſtate of complete fuſion; on the contrary, the following facts, which are to be found in the beſt writers on volcanos, render the Wernerian opinion, viz. that the fluidity of lava has rarely exceeded the ſtate of viſcidity, extremely probable.

Wernerian opinion, that lava is ſeldom more than viſcid; ſupported by its ſlow deſcent,

1. Lava flows very ſlowly, even over conſiderably inclined planes.

2. In the European volcanos glaſs has hardly ever been obſerved. The Obſidian of Iceland, as deſcribed by Bergman and Olafsen; that of Lipari; the glaſs of Veſuvius mentioned by Brierlac, are moſt diſtinctly products of water, as can be demonſtrated by the Wernerian geognoſie.

and the ſcarcity of glaſſy matter,

3. The chryſtals of hornblende and felſpar, which are ſo frequently found in lava, being wrapped up in it (eingewickelt),

prove, as Werner has well obſerved, that theſe are the unaltered chryſtals of the mother-ſtone, which the volcanic fire has not had power to melt. the unaltered chryſtals it contains.

4. When porphiritic baſalt is brought to the ſtate of viſcidity, the chryſtals of felſpar float in it unaltered, and the maſs which is now ſufficiently liquid to flow, however quickly cooled, does not aſſume the glaſſy character, but has the character of the commoneſt kinds of lava. Quick cooling of porphyritic baſalt from its viſcid ſtate does not afford the glaſſy character.

ROBERT JAMESON.

VII.

Deſcription of the Blow-Pipe Apparatus of HAAS. *From a Correſpondent.*

To Mr. NICHOLSON.

DEAR SIR,

AFTER I had communicated to you the apparatus for the blow-pipe by means of alcohol, as uſed by Profeſſor Pictet, which you favoured with a place in the laſt number of your excellent Journal, I was ſtruck with the idea that the apparatus of Mr. Haas, which I mentioned in my letter, though made in the midſt of London, is almoſt entirely unknown in this country. As this laſt is very ingenious, and affords many advantages in the uſe of the blow-pipe, I think you will render a ſervice to your chemical readers by making them acquainted with its nature and conſtruction. In conſequence of this, I here ſubjoin a deſcription and drawing of it, given by Mr. Orſtein in Dr. Scherer's Chemical Journal, Vol. II. page 454. I leave it to your diſcretion whether you think it deſerving a place, and continue with the higheſt eſteem, Letter.

SIR,

Your moſt humble
and obedient,
N. N.

P. S. Concerning the older conſtructions of the blow-pipe, Dr. Scherer refers in that paper to Bergman's Opuſc. II. Engſtröm's Writers on the blow-pipe.

Engſtröm's deſcription of a mineralogical pocket laboratory (which may be ſeen in the ſecond volume of Magellan's tranſlation of Cronſted's Mineralogy, 8vo, Lond. 1788), and to Weigel's Verſuch einer Geſchichte Eſſay towards a hiſtory of the blow-pipe, and its uſe, in Crell's addition to the Chemical Annals, Vol. IV. page 262—283, and page 393—419; alſo, Vol. V. page 6—24, and page 198—226.

Deſcription of the apparatus.

THE delineation here added is upon the whole ſo diſtinct, that a few parts only require a detailed deſcription. I begin with the candle tube, as it is the moſt eſſential part of this apparatus, on which all the others in ſome manner depend.

Plate VII. A is this candle-tube, which may be ſlided backwards and forwards upon the plate B. The rack in the tube ſerves to raiſe the candle. The ball *s*, together with its tubes *c c*, may alſo be raiſed and lowered in order to direct the point *e* accurately towards the flame. The poſſeſſors of this apparatus will undoubtedly thank Mr. Haas for the ingenious arrangement of the inner parts of the ball, by means of which no moiſture can enter into the point *e*.

The charcoal-ſtand C is likewiſe moveable upon B; but at the ſame time, as the drawing ſhews, its pillars *k k* are likewiſe, by means of the ſliding-pieces *i i*, capable of a lateral motion; beſides that at *o* the coal can be vertically turned upon the pillars.

Peculiar conſtruction of ſnuffers.

The conſtruction of the ſnuffers, and the rectangular ſituation of their point *m* with their flaps to the handle *n*, will be found particularly important and uſeful. For, by reaſon of this form, the wick can, without any inconvenience to the hand, be always cut off in the ſame direction in which the flame is blown, and by ſtroking the wick with the point *m* (turned towards the body of the operator), it may be ſo managed as to be always kept broader in the direction of the flame. This will be found more expedient than the uſual method of bending the wick according to the flame's direction. The handle is laid hold on with the thumb and middle finger at *r r*. The fore-finger, for the purpoſe of ſnuffing the candle, opens or ſhuts the moveable flap by puſhing the piece V to the right or left. It is becauſe a common pair of ſnuffers, in the uſe of this apparatus, would be not only extremely incommodious but even detrimental, that Mr. Haas invented this commodious and uſeful inſtrument.

The following articles which make this apparatus complete for uſe, were ſold by Mr. Haas * in a neat box: 1. Various candles; 2. Points of different bores; 3. A hammer; 4. A round excavated piece of ſteel to pound the products of the operation; 5. A pair of tongs; 6. A file; 7. A knife; 8. A magnifying lens; 9. A ſpoon made of platina; 10. A ſteel to ſtrike fire, ſerving at the ſame time as a magnet; 11. A quadrangular magnet bar; 12. A thermometer; 13. An hydrometer.

Additional apparatus.

VIII.

Note reſpecting the Inſtruments by which the two Kinds of Electricity are diſtinguiſhed, or its Direction aſcertained. W. N.

CONCERNING the phenomena denominated electric, the facts have hitherto afforded us little more than a few general laws, which certain ingenious philoſophers have connected by thoſe hypotheſes which conſtitute the theory. Among theſe the two principal are, 1*ſt*, that a peculiar fluid, called the electric matter, exiſts in or upon all bodies; capable of being accumulated by friction, eaſily and moſt rapidly paſſing through or along metals, water, and charcoal, and difficultly or not at all through glaſs and other bodies not containing fluid water; capable of exiſting at the oppoſite confines of non-conductors, ſo as to be in exceſs or plus at one ſurface, and in defect or minus at the other, in much higher degrees than either ſtate could ſingly exiſt; exhibiting the phenomena of ignition and ſound when it paſſes through the ſubſtance of non-conductors; and capable by its rapid motion of raiſing the temperature of conductors to the higheſt intenſity hitherto obſerved, as well as of exciting muſcular action in all degrees, to the entire deſtruction of animal life.

Electricity is yet very imperfectly underſtood.

Franklin's theory of plus and minus.

2. The ſecond received theory of electricity admits the exiſtence of two fluids capable of being ſeparated by friction; having the ſame habitudes, as to conductors and non-conduc-

Nollet's theory of two fluids.

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* Mr. Haas is no longer in London: Five ſets of this apparatus were ſent to Portugal, where he is now eſtabliſhed. The blowpipe and reſt of the adjutage are made of braſs, except the ſnuffers. —Tranſl.

tors, as the ſimple fluid; ſtrongly attracting each other, and not perceptible when combined in a due proportion; capable of exiſting ſeparately at the confines of non-conductors in very high condenſation; cauſing light and ſound when they ruſh together through the ſubſtance of a conductor; and producing the higheſt temperature when they paſs through or meet in non-conductors, &c.

According to the theory of plus and minus, that inſtrument which ſhews the kind will alſo diſtinguiſh the courſe of electricity.

Without entering farther into theſe theories, or the additions they require in order to ſatisfy the facts, ſuch as the attraction ſuppoſed to exiſt between conductors and the electric matter, the atmoſpheres of electricity, &c. the ſubject of the preſent notice requires we ſhould uſe the language of one of the two. The former is moſt commonly adopted; and accordingly we ſhould ſay that an inſtrument which ſhall diſtinguiſh the plus from the minus ſtate of electricity, will alſo ſhew the current or direction in which that matter is moved or carried.

Propoſed means of doing this;

The ſupporters of the theory called Franklin's, from the name of the philoſopher who invented or at leaſt explained it at an early period, have always been aware of the advantage it would be to their ſyſtem if they could adduce any experiment by which the direction of their electric fluid might be aſcertained. Dr. Franklin himſelf very modeſtly offers conjectural reaſons, why the one kind of electricity ſeemed to be a redundancy and the other a deficiency, chiefly grounded on the luminous appearances at the extremity of the wires in the electric ſtates. Henly likewiſe obſerved the lights of the two electricities in the figure of the ſpark, of which the ſtem is always at the plus conductor and the ramifications at the negative; ſo that the forked extremity of lightning will always denote the receiving body, whether it be the cloud or the earth. He alſo uſed his exhauſted glaſs tube with good ſucceſs, as an inſtrument for the ſame purpoſe in which a receiving ball has a luminous atmoſphere, and a giving ball throws out ſtreams of light. The ſame philoſopher firſt noted, that the flame of a ſmall candle is blown towards a negative conductor, and from a poſitive one; though this experiment is not quite unequivocal: And he made ſeveral other experiments which cannot with conſiſtent brevity be here detailed, having this leading object in view.

from electric light; figure of the ſpark; Lightning; exhauſted tube; flame of a candle;

None of theſe afford any deciſion

Among the experiments made with this view, every one of which, it muſt be confeſſed, leave the great queſtion of the nature of the real nature and courſe of electricity.

Two inſtruments for the ordinary purpoſe; viz. the Galvanic tube;

commodiouſly formed;

nature and direction of the ſuppoſed electric matter undetermined, there ſeems to be only two which afford the ſimple apparatus we are in the habit of diſtinguiſhing by the name of inſtruments. As both inſtruments appear to deſerve more immediate notice than they have yet had, I have inſerted them in Plate VIII, where Fig. 4 repreſents the Galvanic tube, which, by the ſkilful management of Dr. Wollaſton, is rendered capable of diſtinguiſhing the courſe of electricity. The ingenious Mr. Acum, of Compton Street, Soho, who ſupplies chemiſts and philoſophers with apparatus and materials of every kind, made the inſtrument where this drawing was taken. Two fine gold wires are fixed in ſeparate ſmall glaſs tubes by fuſion; and the ends of theſe tubes are ground away till the very ſection or extremity of each wire is laid bare: at the other end of each tube appears a larger wire with a ball, which wire communicates with the gold wire within. Theſe two tubes are fixed by fuſion in a larger tube, provided with a conical drawn aperture, through which it can be filled with water, all but a ſmall vacuity or bubble, and then ſealed. The apparatus is then complete; and if a ſtream of electricity or Galvaniſm be paſſed through it, the water is decompoſed, and of the gas thus extricated, the larger ſtream of hidrogen ſhews the minus ſide. It is probable this inſtrument might be of uſe in atmoſpherical obſervations, in caſe large ſtreams of electricity ſhould paſs through an elevated conductor at a low intenſity.

— and an apparatus with a low point.

The other inſtrument, Fig. 5, was conſtructed by myſelf about twenty years ago, from an obſervation I made that low points ceaſe to act as ſuch with the plus at higher elevations, than with the minus electricity. So that if two balls be inſulated, and a low point proceeds from one of them, electricity will fly through the interval in a ſpark if the point be plus, but will be ſilently tranſmitted if the point be minus.

This inſtrument, like the other, may be applied to diſtinguiſh the atmoſpheric changes; but it will be effectual only when the electricity is ſtrong enough to give ſparks.

Section

IX.

Section and Plan of a moveable Crane, capable of heaving four Tons with four Men. Deſigned and executed for the Pier at Ramſgate, by Order of the Truſtees for the Management of the Harbour at that Place. By Mr. PETER KEIR, *of St. Pancras. Communicated by Mr.* P. KEIR.

To Mr. NICHOLSON.

SIR,

ccount and de-ription of the ane.

THE crane repreſented in the annexed drawings was conſtructed at the requeſt of the Ramſgate Harbour Truſt. It conſiſts of a caſt iron frame, marked A, Plates V. and VI. weight two tons, and 9 feet 7 inches ſquare, ſupported on four caſt iron wheels, two of which traverſe on a centre at B, by the rack work C, to ſteer it when moving from place to place. A caſt iron upright ſhaft, D, weight 23cwt. ſupported by oak braces; the weight of the framing and wheel work is ſupported by a ſteeled pivot on the top of the ſhaft at F, round which the whole ſwings, including the men that work it.

The drawing is ſufficiently plain, to require no farther explanation. It has been executed, and may be inſpected at any time on Ramſgate Pier.

I am, SIR,

Your obedient humble ſervant,

PETER KEIR.

arther particu-rs.

Upon inquiry, I find that the original inſtructions were to make a crane capable of raiſing four tons, and that the centre piece, or upright ſhaft, was at firſt made of oak: but that the ſtrain for diſengaging goods from their ſtowage requiring the occaſional exertion of a greater power, it was found adviſeable to ſubſtitute the preſent iron piece. The coſt of this engine was four hundred pounds; but from the above circumſtance, and particularly becauſe the expence of patterns, &c. will not again be requiſite, I am informed that others might now be made for three hundred pounds each.

W. N.

Experiments

X.

Experiments on the Separation of Light and Heat by Refraction. In a Letter from Sir H. C. ENGLEFIELD, *Bart. F. R. S. to Thomas Young, M. D. F. R. S. From the Journal of the Royal Inſtitution, p.* 202.

DEAR SIR,

Letter to Dr. Young.

IN purſuance of your deſire, I communicate to you the experiments which I made in conſequence of Dr. Herſchel's moſt intereſting diſcovery of the ſeparation of ſolar heat and light by the priſm. They carried with them, to my mind, a complete conviction of the truth and accuracy of the Doctor's aſſertions on that ſubject. Should you judge them worthy of inſertion in the Journals of the Royal Inſtitution, I ſhall think myſelf honoured by their publication.

I am, Dear Sir,
Your faithful Servant,
H. C. ENGLEFIELD.

Introductory remarks.

As I was deſirous not only to aſcertain, by actual obſervation, the very curious diſcovery of Dr. Herſchel, but to obviate the objection made by Mr. Leſlie * to the mode in which his experiments had been made, I contrived an apparatus totally different in its arrangement from that which had been uſed by the Doctor; and diſpoſed in ſuch a manner that it was impoſſible that the thermometers ſhould be affected by any foreign heat reflected from any part of it. As to the heat reflected from the floor, it could not influence the reſults, ſince it was the ſame, whatever colour was thrown on the ball of the thermometer.

A priſm was placed in the ſolar light in a room not darkened.

As I had nothing to do with light, it was not neceſſary to darken the room; and as I wiſhed to accumulate as large a portion of ſolar heat as poſſible, I placed the priſm in an open window, inſtead of admitting a beam through an aperture in a ſhutter, as is the uſual practice. The priſm I uſed is a very good one; and it was lent to me for this purpoſe by Mr. Walker, of Conduit-Street. It is three inches long, and equilateral, each ſide being 1.15 inch broad.

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* In his communications to the Philoſophic Journal (quarto) Vol. IV. p. 344, 416.

It was ſupported by an horizontal arm, projecting from a pole like that of a fire-ſcreen. The arm could be adjuſted to any height by a ſcrew; and the priſm was likewiſe capable of being turned on its axis to any required poſition.

The ſpectrum fell on a lens.

The coloured ſpectrum was thrown on a very good lens, of four inches in diameter, and about 22 inches focal length. This lens formed part of a machine well known, and uſed for viewing prints; it ſtands on a foot adjuſtable in any direction, and to any height; and the mounting being of wood, and merely ſufficient to ſupport the lens, ſcarcely any heat can be accumulated in any part of it. The whole lens, as well as its mounting, was covered with a thick white paſteboard ſcreen, in which was cut a ſlit of 3 inches long, and half an inch wide; this ſlit was over the centre of the lens; and through it any one of the colours required was admitted on the lens, while all the reſt of the ſpectrum was totally excluded. A light wooden arm, of two feet long, projected at right angles from the lower part of the mounting of the lens. This arm carried a ſmall ſcreen of poliſhed card, which received the image formed in the focus of the lens. This was found neceſſary, in order to find with certainty where to place the thermometer; when the focal diſtance was found, the ſcreen was moved back, about a diameter of the bulb of the thermometer uſed, which was then held by the hand in the focus of the lens, which was done with great eaſe and certainty, as nothing more was neceſſary than to fix the eye on the card ſcreen, and keep the ball of the thermometer in the centre of the luminous image. The whiteneſs and poliſh of the ſcreen totally precluded any accumulation of heat in it; nor indeed would ſuch accumulation have been of any detriment to the experiments had it exiſted; for, as it muſt have been entirely owing to the ray under examination, it would have done nothing more than increaſe the effect of it on the thermometer.

Through a ſlit which ſuffered one colour only to paſs.

A mercurial thermometer was held in the focus.

The thermometers uſed were mercurial, and very ſenſible. The ſcales were ivory tubes, embracing the ſtem, and graduated within. The ball of the inſtrument was therefore unconnected with any mounting; and no falſe heat could poſſibly affect it. The balls were moſtly blackened with Indian ink, carefully laid on; but ſome were uſed naked, and one, covered with white water-colour paint,

The

Poſitions of the lens and priſm.

The lens, with its apparatus, was placed about three feet from the priſm; and, as the ſun was pretty high in the greater part of the experiments, and the deſcending ſpectrum was in general uſed, the floor under the ſtand of the lens was in ſhade, from the wall below the window, and had been ſo all the day, as the window at which moſt of the experiments were made, fronted the ſouth

Some part of this detail might ſeem ſuperfluous, had not ſuch ſtreſs been laid on the ſuppoſed accumulation of heat in Dr. Herſchel's experiments, that it was neceſſary to obviate any objection on that head; or to ſhow, at leaſt, the utter improbability of any cauſe of that nature having affected the reſults of theſe experiments.

I now proceed to relate the experiments themſelves: which I ſhall tranſcribe from the original notes made at the moment.

April 6, 1801.

Experiment I.
Maximum riſe of temperature of the therm.
in blue = 1°
green = 4
yellow = 6
red = 16
conf. r. = 15½
beyond = 18

The apparatus being diſpoſed as above deſcribed, the coloured rays of the deſcending ſpectrum of the priſm were ſucceſſively thrown on the ſlit in the ſcreen, covering the lens; and the thermometer with a blackened ball, placed in the focus of the lens, roſe as follows:

In the blue in 3′, from 55° to 56°. or 1°.
Green in 3′, from 54° to 58°. or 4°.
Yellow in 3′, from 56° to 62°. or 6°.
Full red in 2½′, from 56° to 72°. or 16°.
In the confines of the red in 2½′, from 58° to 73½°. hr 15½°.
Quite out of viſible light in 2½′, from 61° to 79°. or 18°.

Between each of the obſervations the thermometer was placed in the ſhade ſo long as to ſink it below the heat to which it had riſen in the preceding obſervation: of courſe its riſe above that point could only be the effect of the ray to which it was now expoſed *. A thermometer placed conſtantly in the ſhade near the apparatus, ſcarcely varied during the experiments.

April 17th, 11, A. M.

Three thermometers, uſed afterwards in the experiments, were expoſed to the ſun's rays until they became ſtationary.

The

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* In all the experiments the thermometer was continued in the focus long after it had ceaſed to riſe; therefore the heats given are the greateſt effect of the ſeveral rays on the thermometer in each obſervation.

The thermometer with

Naked ball	58½°
Whitened ball	58½°
Blackened ball	63°

Experiment II.

The apparatus being placed all as before, the blackened thermometer

Black ther. roſe
in red = 3°
dark = 5
White therm.
in red 3°.
dark 0½
Black ther.
in dark 4°

In full red ray in 3′, from 58° to 61°. or 3°.
In quite dark in 3′, from 59° to 64°. or 5°.
The whitened thermometer
In full red ray in 3′, from 55° to 58°. or 3°.
In quite dark in 3′, from 58° to 58½°. or 0½°.

The blackened thermometer was now again placed in the quite dark, and roſe in 3′, from 58° to 61°. or 4°.

This is what I expected, as a thick ſmoky haze had come on ſince the experiments were begun, and increaſed much towards the end of them.

Experiment III.
Black ther. roſe
in confine of red
12°
White do. do. 3°

April 18, 11, A. M. Sun clear. All the apparatus as before.
In the confines of the red.
Black thermometer in 3′, from 59° to 71°.
White thermometer in 3′, from 57½° to 60½°. Clouds came on, and put a ſtop to the experiments.

Experiment IV.
Black ther. roſe
in full red 12°
White do. do.
3½°.

April 19, 3½, P. M. Sun clear.
In the full red ray.
Black thermometer in 3′, from 66° to 82°
White thermometer in 3′, from 66° to 69½°.

In confine of red
Black th. 12½°
Ditto, quite dark
14°

In the confines of the red.
Black thermometer in 3′, from 67° to 79½°. But thin ſtreaky clouds had come over the ſun.
In quite dark, half an inch out of the red.
Black thermometer in 3′, from 70° to 84°.

Tranſitions from
red to dark occa-
ſioned changes of
temperature.

When the thermometer was carried into the faint red light, it ſunk quickly; and roſe again as quickly when carried into the dark focus: but when carried into the dark on the other ſide of the red light, it ſunk very rapidly, and did not appear to receive any heat at all. Thin clouds increaſed, and rendered the ſun's light too faint for further experiments.

Experiment V.

April 20, from 10½ to 11½, A. M. Sun quite clear.

Although it could not be ſuppoſed that effects of the refracted light could differ in the two ſpectra, yet, in order to aſcertain the fact, the horizontal ſpectrum was uſed in the ſubſequent experiments.

The apparatus all the ſame as in the former.

Black therm.
full red 4°½
dark 9½°

In the full red ray.

Black thermometer in 3′, from 67° to 71½°

Quite out of the ray.

Black thermometer in 3′, from 68° to 77½°.

Darker 10½°.

The ray was now ſo far removed from the ſlit in the ſcreen, that ſcarce any light was perceptible in the focus of the lens. The black thermometer was now placed near half an inch from the bound of the viſible light in the focus, and roſe in 3′, from 69° to 79½°.

Moſt dark 9°.

The utmoſt edge of the priſmatic ſpectrum was now removed an eighth of an inch from the edge of the ſlit in the ſcreen; and no light was now viſible in the focus of the lens.

Black thermometer in 3′, from 70° to 79°.

The difference perceivable by the touch.

Mr. Cary, optician in the Strand, and Dr. Hunter, were preſent at theſe experiments, and repeatedly ſaw the thermometer, in the ſecond experiment, ſink when carried into the light, and riſe again when removed back into the dark. Dr. Hunter alſo received the focus on the palm of his hand, where the heat was ſenſibly felt; and on ſhutting his eyes, and pointing with a long pen to where the heat was greateſt, he always touched his hand beyond the viſible light.

Deſcription of the red image.

As the red image has been continually mentioned in the courſe of the above recited experiments, it may not be improper to deſcribe it more particularly. The diameter of the red ſpot, formed by the ray in the focus of the lens, was juſt two tenths of an inch in diameter, at right angles to the length of the ſpectrum, and well defined: in the direction of the ſpectrum it was elongated, as might be expected, and leſs well defined.

Very faint red in the focus when the heat was greateſt.

When the whole viſible ſpectrum from the priſm was received on the ſcreen which covered the lens, and the utmoſt edge of the red rays was removed a full eighth of an inch from the edge of the ſlit in the ſcreen, there was ſtill a faint bluſh of red, of a ſemioval form, viſible when the focus of the lens was thrown on a white ſcreen; and it was in theſe circumſtances that the greateſt effort of heat was conſtantly produced on the thermometer, not by placing it in the red light, but out of it, in the axis of the lens.

Repetition of experiments.

I have only to add, that in the courſe of the month of June 1802, I repeated moſt of theſe experiments with the ſame

apparatus,

apparatus, in preſence of Mr. Davy, with the moſt complete ſucceſs; the ſun's altitude being greater, the effect of his rays was ſo great as to raiſe the thermometer in the inviſible ray to 98°, while the viſible red never raiſed it above 87°. At the ſuggeſtion of Mr. Davy, we tried ſeveral experiments with reſpect to the power of the ſeveral coloured rays in rendering Canton's phoſphorus luminous; and we found, without a poſſibility of doubt, that the blue rays poſſeſſed that power in a much higher degree than the red. There was great reaſon to ſuſpect that this power, like that of blackening the nitrate of ſilver, extended beyond the viſible blue ray; but our apparatus was not prepared for the more delicate part of theſe experiments, which are only mentioned with a view of exciting further reſearches on this very intereſting ſubject, and of giving to Mr. Davy the credit due to him for having firſt thought of the experiment.

The blue rays illuminate Canton's phoſphorus more than the red

XI.

*On the Expanſion of the Elaſtic Fluids by Heat.** *By* Mr. John Dalton.

Occaſion of the Experiments and Eſſay.

THE principal occaſion of this eſſay is another on the ſame ſubject by Meſſrs. de Morveau and du Vernois in the firſt vol. of the Annales de Chimie. It appearing to them that the reſults of the experiments of De Luc, Col. Roi, de Sauſſure, Prieſtley, Vandermonde, Berthollet and Monge did not ſufficiently accord with each other; and that it would be of importance to determine not only the whole expanſion of each gas from two diſtant points, ſuch as the freezing and boiling, but likewiſe whether that expanſion be uniform in every part of the ſcale, they inſtituted a ſet of experiments expreſsly for thoſe purpoſes. The reſult of which was, that betwixt the temperatures of 32° and 212°, the whole expanſion of one gas differs much from that of another, it being in one inſtance about $\frac{4}{10}$ of the original, and in others more than 12 times that expanſion; and that the expanſion is much more for a given number of degrees in the higher than in the lower

Experiments of Guiton and Du Vernois.

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* Mancheſter Memoirs, V. 595.

part of the ſcale. Theſe concluſions were ſo extremely diſcordant with and even contradictory to thoſe of others, that I could not but ſuſpect ſome great fallacy in them, and found it in reality to be the fact: I have no doubt it aroſe from the want of due care to keep the apparatus and materials free from moiſture.

The author's method with a ſimple manometer tube.

My method of experimenting on this ſubject is ſimple, and therefore leſs liable to error. A ſtraight manometer tube, ſuch as has been mentioned, is duly divided into equal portions of capacity; it is then dried by a wire and thread, and the open end inſerted through a cork into a phial containing ſulphuric acid, in order that the aqueous vapour may be drawn out of the tube; this is eſſential if we operate in temperatures lower than that of the atmoſphere, otherwiſe not. For want of this attention, Col. Roi, in his valuable paper in the Philos. Tranſ. vol. 67, has been led into ſome erroneous concluſions. A ſmall column of dry mercury is then let down to a proper point in the manometer, and it is ready for experiment with common air.

and air dried by ſulph. acid.

Method of filling the manometer.

It requires ſome addreſs to fill the manometer with any other gas.—I ſucceeded beſt as follows: filled the tube with dry mercury; then puſhed down a wire with thread, ſo that when the wire was got to the end of the tube, a thick covering of thread juſt entered the open end, and held the mercury like a cork, ſo that the tube could be inverted without loſing the contents; then having a glaſs funnel with a perforated cork over the water apparatus, containing the gas, I ſlipped the manometer through the hole in the cork, and putting my hand into the water under the funnel, drew the wire out of the manometer, and with it the mercury; upon which the gas entered the manometer. For carbonic acid gas, I opened the ſealed end of the manometer, drew it out to a capillary bore, and forced a ſtream of the gas through the tube; then putting my finger on the other end, ſealed it again by a blow-pipe, and let down a ſmall column of mercury to the proper point.

Carbonic acid gas.

Simple method of applying the boiling heat.

When the manometer was to be expoſed to a heat of 212°, I uſed a Florence flaſk, with a long glaſs tube corked into it, in ſuch ſort that as much of the manometer as was neceſſary to be expoſed to the temperature might be in the tube; then the water at the bottom of the flaſk was made to boil violently, ſo that a conſtant ſtream of vapour iſſued out of the top of the glaſs tube, which was found to raiſe the thermometer to 212°. Small ſpecks of white paint were put upon the diviſions of the manometer together with numbers which were diſcernible through the containing tube. For lower temperatures, a deep tin veſſel containing hot water was uſed, in which the manometer was immerſed, the water being well agitated previouſly to each obſervation.

The concluſions of De Luc, Roy, Berthollet, &c. are accurate; but thoſe of Morveau and du Vernois not ſo.

From a great many experiments made in this way on common air, and likewiſe upon hydrogenous gas, oxygenous and nitrous gaſes, and carbonic acid gas, I can aſſert that the concluſions of De Luc, Roi, Sauſſure, Berthollet, &c. are nearly accurate throughout, and that thoſe of de Morveau and du Vernois are extremely inaccurate in the higher temperatures.

Expanſion of common air.

I have repeatedly found that 1000 parts of common air of the temperature 55° and common preſſure, expand to 1321 parts of the manometer; to which adding 4 parts for the correſponding expanſion of glaſs, we have 325 parts increaſe upon 1000 from 55° to 212°; or for 157° of the thermometric ſcale. As for the expanſion in the intermediate degrees, which Col. Roi's experiments ſhew to be a *ſlowly diminiſhing* one above the temperature of 57°, but which de Morveau's on the contrary ſhew to be a *rapidly increaſing* one in the higher part of the ſcale; I am obliged to allow that Col. Roi is right, though it makes in ſome degree againſt an hypotheſis I have formed relative to the ſubject; he has certainly however made the diminution too great from 72° downwards, owing to his not perceiving that he actually *deſtroyed* a portion of the elaſtic fluid he was operating upon (aqueous vapour) in reducing its temperature ſo low; if his air had been previouſly dried by ſulphuric acid, &c. he would not have found ſo remarkable diminution below 72°. My experiments give for $77\frac{1}{2}$° above 55°, 167 parts; for the next $77\frac{1}{2}$° only 158 parts; and the expanſion in every part of the ſcale ſeems to be a gradually diminiſhing one in aſcending.

Intermediate expanſion.

Other gaſes agree with common air.

The reſults of ſeveral experiments made upon hydrogenous gas, oxygenous gas, carbonic acid gas and nitrous gas, which were all the kinds I tried, agreed with thoſe on common air not only in the total expanſion, but in the gradual diminution of it in aſcending: the ſmall differences obſerved never exceeded 6 or 8 parts on the whole 325; and differences to this amount will take place in common air, when not freed from aqueous vapour, which was the ſituation of all my factitious gaſes.

Upon the whole therefore I ſee no ſufficient reaſon why we may not conclude, that *all elaſtic fluids under the ſame preſſure expand equally by heat*,—and that *for any given expanſion of mercury, the correſponding expanſion of air is proportionally ſomething leſs, the higher the temperature.*

The law is general for all elaſtic fluids.

This remarkable fact that all elaſtic fluids expand the ſame quantity in the ſame circumſtances, plainly ſhews that the expanſion depends *ſolely* upon heat: whereas the expanſion in ſolid and liquid bodies ſeems to depend upon an adjuſtment of the two oppoſite forces of heat and chemical affinity, the one a *conſtant* force in the ſame temperature, the other a *variable* one, according to the nature of the body; hence the unequal expanſion of ſuch bodies. It ſeems therefore that general laws reſpecting the abſolute quantity and the nature of heat, are more likely to be derived from elaſtic fluids than from other ſubſtances.

Suppoſed cauſe they have to counteracting attraction.

In order to explain the manner in which elaſtic fluids expand by heat, let us aſſume an hypotheſis that the repulſive force of each particle is exactly proportional to the whole quantity of heat combined with it, or in other words to its temperature reckoned from the point of total privation: then, ſince the diameter of each particle's ſphere of influence is as the cube root of the ſpace occupied by the maſs, we ſhall have $\sqrt[3]{1000} : \sqrt[3]{1325}$ (10 : 11, nearly) : : the abſolute quantity of heat in air of 55°: the abſolute quantity in air of 212°. This gives the point of total privation of heat, or abſolute cold, at 1547° below the point at which water freezes. Dr. Crawford (On Animal Heat, &c. page 267) deduces the ſaid point by a method wholly different to be 1532°,—So near a coincidence is certainly more than fortuitous.

Hypotheſis to explain the law of expanſion.

Singular coincidence as to the abſolute zero.

The only objection I ſee to this hypotheſis is, that it neceſſarily requires the augmentation of elaſtic fluids for a given quantity of heat to be greater in the higher temperatures than in the lower, becauſe the cubes of a ſeries of numbers in arithmetical progreſſion differ more the larger the numbers or roots: but it has juſt been ſhewn that in fact an augmentation of a contrary kind is obſerved. This refers us to the conſider-

An objection if we admit the expanſion of mercury to be as the heat; but not ſo if mercury follow the law of all other fluids.

ſideration whether the mercurial thermometer is an accurate meaſure of the increments of heat; if it be, the hypotheſis fails; but if equal increments of heat cauſe a greater expanſion in mercury in the higher than in the lower temperatures, and that in a ſmall degree, the fact noticed above inſtead of being an objection will corroborate the hypotheſis.—Dr. Crawford determines the expanſions of mercury to be very nearly in proportion to the increments of heat: M. De Luc makes them to be leſs for a given quantity of heat in the lower than in the higher part of the ſcale; and in a ratio that agrees with this hypotheſis. Now as every other liquid we are acquainted with is found to expand more in the higher than in the lower temperatures; analogy is in favour of the concluſions of De Luc, that mercury does the ſame.

Short account of ſimilar reſearches by Mr. Gay Luſſac.

In the Bulletin des Sciences, there is a notice of a memoir of Cit. Gay Luſſac on the dilation of the gaſes and vapours; and the memoir itſelf is given in the 43d volume of the Annals de Chémie, whence I ſhall, as early as convenient, give either a full abſtract or a tranſlation. This able author gives the ſame concluſion as that of Mr. Dalton, and, like him, attributes the errors of preceding experimenters to the humidity of the gaſes they examined.

His method.

His experiments were made by expelling the gas from a glaſs ball properly fitted up, and determining the expanſion by weighing the apparatus, after water had been ſuffered to come in during the cooling, to occupy the ſpace of the extruded gas. He very candidly informs us, that Cit. Charles had deduced the general reſult of the equal and ſimilar expanſion of all the gaſes by heat, under like preſſure, fifteen years before, though he did not publiſh it; and he gives an hiſtorical account of the labours of his predeceſſors; ſo that we muſt neceſſarily conclude that he could not have ſeen the late publication of the Mancheſter Society, in which the extenſive reſearches of Mr. Dalton are inſerted; and wherein it appears that the preceding memoir was read about a year before that of Mr. Gay Luſſac was read to the Inſtitute. The French Philoſopher finds the expanſion or increaſe of dimenſion, between the freezing and boiling water points, to be 0.375, which

Mr. Dalton's experiments were earlier.

which gives for the ſpace between 55° and 212°, or 157°, the quantity 0.327 inſtead of 0.321, as Mr. Dalton finds it. This difference of ſomewhat leſs than one fiftieth part of the expanſion, may perhaps have ariſen from a difference in the ſcales of operation, of perhaps the difficulty of producing a given temperature through the whole maſs of a fluid, &c.

How far they agree.

XII.

Account of ſome Experiments made in the Laboratory of the Royal Inſtitution, relating to the Agencies of Galvanic Electricity, in producing Heat, and in effecting Changes in different fluid Subſtances. By H. DAVY, *Prof. Chem.*

I. It has been ſhown, by a very intereſting experiment made in France, by Meſſrs. Fourcroy, Vauquelin, and Thenard, that the power of galvanic batteries containing large plates, to ignite metallic ſubſtances, is much greater than that of batteries compoſed of an equal number of ſmall plates; though their agencies upon water, and upon the human body, are nearly the ſame.

Diſcovery of Fourcroy, Vauquelin, and Thenard, of the greater ignition by large galvanic plates.

In examining the circumſtances of the action of a galvanic apparatus, or trough, conſtructed in the Royal Inſtitution, and containing twenty ſeries of plates of copper and zinc, ſquare, and thirteen inches in diameter, I obſerved that the ſame relations between chemical agency and the production of galvanic electricity exiſted as in other caſes. When pure water was uſed for filling the cells, the ſparks, as well as the ſhocks, were extremely indiſtinct; and the battery was capable of igniting only about a line of iron wire of $\frac{1}{170}$ of an inch in diameter. With ſolution of muriate of ſoda it acted better; and diluted nitric acid was ſtill more efficacious. With this laſt ſubſtance, it became capable of rendering white-hot three inches of the iron wire of $\frac{1}{170}$, and of cauſing two inches to enter into fuſion.

The trough with large plates has the ſame relation of chemical agency as other apparatus.

Water had little effect; ſalts and acid much more.

In comparing the effects produced by a ſolution of nitrous acid, of the ſpecific gravity of 1.4, in about ſixty parts of water, with thoſe occaſioned by a concentrated ſolution of carbonate of potaſh, the acid was found to produce by much the greateſt intenſity of action; which can hardly be aſcribed to any

Nitrous acid did not act *by its conducting power*. For it is much *more* powerful than carbonate of potaſh, though it conducts much *leſs*.

any other cauſe than its chemical agency; for, with regard to conducting power, it appeared very much inferior to the other ſolution. There is every reaſon to believe, that with pure water, that is, water deprived of air and of all ſaline ſubſtances, no action would be produced in this battery. I was unable to aſcertain the fact by a direct experiment; but I found repeatedly, that a pile, compoſed of thirty-ſix ſeries of ſquare plates, of copper and zinc, of five inches in diameter, loſt its activity in nitrogene and hydrogene gaſes, in about two days; and it was conſtantly reſtored by common air; and rendered more intenſe by oxigene gas.

Water appears to act, not as a conductor, but by its gas.

Water and oil made to boil by galvaniſm.

II. When the galvanic battery, with large plates, was in full action, it was found that a - $\frac{1}{80}$ of an inch in diameter, and two feet long, when placed in the circuit, was rendered ſo hot as to cauſe a ſmall quantity of water, brought in contact with it, ſpeedily to boil. It continued warm for many minutes; and, by an occaſional momentary interruption and completion of the circle, the heat was permanently kept up. When three or four inches of the wire of $\frac{1}{170}$ were placed in any part of the conducting chain, they continued red-hot for more than a minute; and, by a ſucceſſion of interruptions and contacts, they were kept partially ignited for five or ſix minutes. When that part of the communicating chain containing the ſmall wire was introduced into a ſmall quantity of ether, alcohol, or oil, the fluid ſoon became warm; and olive oil, the only ſubſtance that was expoſed for a ſufficient time, was made to boil,

Charcoal made red hot under water, oils, &c. with the extrication of gaſes.

III. When two ſmall pieces of well burned charcoal, or a piece of charcoal and a metallic wire, were made to complete the circle, in water, vivid ſparks were perceived, gas was given out very plentifully, and the points of the charcoal appeared red-hot in the fluid, for ſome time after the contact was made; and, as long as this appearance exiſted, elaſtic fluid was generated, with the noiſe of ebullition. The *ſenſible* phenomena were nearly the ſame with the volatile and fixed oils, ether, and alcohol; and, by means of charcoal, the ſpark could be produced in concentrated ſulphuric and nitric acids, which are amongſt the beſt of the leſs perfect conductors.

Nature of the gaſes;

The gaſes produced from different fluids by the galvano-electric ſpark, were examined; and as the reſults were, in moſt caſes, what might have been expected from theory; the analyſis of them was not made with very minute attention.

from water;

When water was acted upon by ſparks taken from two pieces of charcoal, the elaſtic products evolved were about $\frac{3}{8}$ of carbonic acid, $\frac{1}{8}$ of oxigene, and the remainder an inflammable gas, which required a little more than half its volume of oxigene for its combuſtion. With gold and charcoal, the gold being on the zinc ſide, the gas produced appeared to be chiefly a mixture of oxigene and hidrogene, for it diminiſhed $\frac{7}{10}$ by the electric ſpark.

from alcohol;

The gas diſengaged from alcohol, the ſpark being taken by gold connected with the zinc end, and charcoal, was a mixture of nearly two parts of oxigene and eleven parts of inflammable gas, which appeared to be partly light hidrocarbonate.

ether;

Ether, in the ſame method of operating, gave four parts of oxigene and twelve parts of inflammable gas.

ſulph. acid;

From ſulphuric acid, oxigene and hidrogene were produced very rapidly, (the oxigene being more than ſufficient for the ſaturation of the hidrogene by combuſtion,) and the acid became blue.

nitric acid.

The gas from nitric acid detonated with great violence by the electric ſpark, and the reſiduum was oxigene, mixed with a little nitrogene.

Ignition of the charcoal accounted for.

The products from the acids, there is every reaſon to believe, were evolved chiefly in conſequence of the decompoſition of the water they contained. And, in operating upon theſe ſubſtances, as well as upon pure water, a portion of the elaſtic fluids muſt have been produced at the time of the ſilent tranſmiſſion of the electricity, during the momentary interruptions of contact. The apparent ignition of the charcoal in the different fluids depended, probably, in ſome meaſure, upon its being ſurrounded, at the moment of contact, by globules of gas, which prevented the heat, produced at the points of it, from being rapidly carried off by the fluid.

Gas from fuſed phoſphorus.

When the ſpark was taken by means of iron wires, in phoſphorus rendered fluid by heat, under a ſtratum of water, permanent gas was produced from it, but in a quantity too ſmall to be examined, after a proceſs that continued an hour. I purpoſe to repeat the experiment, with conductors of dry charcoal.

IV.

The large plates diſengage moſt gas; provided the conductor's power be good.

IV. When gold wires, connected with the ends of the battery, were made to act upon fluids in the common method of communication, being placed at a diſtance from each other, it was found that the rapidity of the evolution of the gaſes was much more influenced by the conducting power of the fluid, than it is in common caſes with ſmall plates. In comparing the action of a battery of twenty plates, of five inches in diameter, upon ſulphuric acid, nitric acid, and various ſaline ſolutions, with that of the large battery, it was obſerved, in ſeveral experiments, that the gas was diſengaged much faſter, and in larger quantities, from the wires connected with the large plates, whilſt the action of the two arrangements upon water was nearly the ſame. This fact, combined with other facts of the ſame kind, ſeems to ſhow, that the quantity of electricity excited in the arrangements with large ſurfaces, is much greater than that produced in thoſe with ſmall ſurfaces; and that it is capable of paſſing with facility through the more perfect conductors, whilſt, from the nature of the ſeries, its circulation is impeded, comparatively to a great extent, by imperfect conductors; a conjecture that has been already formed by different philoſophers.

Attempt to produce change in muriatic gas, by the galvanic ignition of charcoal.

V. As the great quantity of electricity made to circulate through perfect conductors, by means of the large apparatus, increaſes their affinity for oxigene more perhaps than any known agent, and as charcoal by means of it can be rendered white-hot, and kept in conſtant combuſtion in oxigene gas or atmoſpherical air, I thought of trying the effects of the electrical ignition of this ſubſtance, upon muriatic acid gas confined over mercury.

Experiment.

The experiment was made by means of a ſmall glaſs tube*, containing a ſlip of platina hermetically ſealed into it, and having a piece of charcoal attached to its lower extremity: the communication was effected by means of iron wires; and the charcoal was made white-hot, by ſucceſſive contacts continued for nearly two hours. At the end of this time, the muriatic acid gas had diminiſhed a very little in volume: much white matter had formed upon the charcoal, which was not ſenſibly conſumed. When the gas was examined, ¾ of it were inſtantly abſorbed by water, and the remainder proved to be

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* For a deſcription of this apparatus, ſee p. 214.

inflam-

Effects;

inflammable. The proceſs was repeated three times; and, when the ſpark was moſt vivid, a white cloud was always perceived at the moment of its production, I am inclined to attribute this phenomenon, and the other phenomena, to the decompoſition of the water held in ſolution in the gas, by the charcoal and the mercury adhering to it; and the white matter was probably muriate of mercury. The acid gaſes are rapidly abſorbed by charcoal; and this ſubſtance, when well made, will take up more than 30 times its volume of muriatic acid gas; ſo that, in the proceſs of ignition, a part of the water and of the acid muſt have been acted upon in a very condenſed ſtate.

very ſimilar to thoſe of Mr. W. Henry with common electricity.

No decompoſition.

The want of ſucceſs in this experiment, the reſults of which are very ſimilar to thoſe obtained by Mr. William Henry*, in his trials with common electricity, prevented me from carrying on the proceſs upon fluoric acid gas, as I had at firſt intended. Many of the compound gaſes that are decompoſable by heated charcoal, might probably, however, be analyſed in a very ſimple manner, by means of the ignition of that ſubſtance by galvanic electricity; and this mode of operating may be conveniently applied, for aſcertaining the relations of the affinities of charcoal for the conſtituent parts of compound gaſes at very high temperatures.

SCIENTIFIC NEWS, &c.

NATIONAL INSTITUTE OF FRANCE.

Prize propoſed at the Public Sitting of the 15 *Germinal, in the* 10*th Year.* (*April* 5, 1802.)

Conditions.

THE general conditions are as uſual; namely, that the name of the author ſhall be concealed, and his rank diſtinguiſhed by a ſentence or device, which ſhall alſo be written on a ſealed billet, containing his name and addreſs. The works are to be addreſſed (carriage paid) to one of the Secretaries of the Inſtitute. Neither the treatiſes, nor any drawings or machines thus forwarded, will be reſtored; but the authors may take copies, and may again receive their models or machines, upon ſubſtituting regular drawings in place of them. The Commiſſioner of the Funds of the Inſtitute will deliver the gold medal to the bearer of the Secretary's receipt, if given for the treatiſe, &c.; but where no receipt exiſts, the author muſt himſelf appear, or ſend his procuration.

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* Phil. Tranſ. 1800, p. 188, or Philoſ. Journal, quarto, iv. 209. 245.

Subject of the Prize of Chemiſtry.

The claſs of mathematical and phyſical ſciences having propoſed in the year 8 the following prize queſtion, to be decided upon at the ſitting above mentioned:

Prize queſtion reſpecting ferments.

What are the characters which diſtinguiſh in vegetable and animal matters thoſe which ſerve as ferments, from thoſe in which they cauſe or excite fermentation.

And none of the memoirs having fulfilled the conditions of the program, the claſs propoſes the ſame ſubject again for the year 12.

The prize will be a gold medal, of the value of one kilogramme, which will be given at the ſitting of the 15 Germinal of the year 12 (April 5, 1804).

Prize Queſtions from the Batavian Society at Haarlem.

Prize queſtions.

The Batavian Society at Haarlem has publiſhed a number of ſubjects of prizes at their ſitting of May 1, 1802; among which are the following:

Reſpecting manures.

1. "How far it is known, from the lateſt diſcoveries in the phyſiology of plants, in what manner the different manures on different ſoils are favourable to the vegetation of plants; and what indications may we deduce from the knowledge required on this ſubject, to direct our choice as to manures, and the fertilization of uncultivated and arid lands?"

This queſtion is continued to the 1ſt of Nov. 1802.

— motion of the ſap.

2. "What is the actual ſtate of our knowledge reſpecting the motion of the ſap in trees and plants? In what manner can we acquire a more complete knowledge reſpecting the obſcure and doubtful parts of the ſubject? And is it poſſible to deduce, by deciſive experiments, ſuch indications as may be uſeful for the cultivation of trees and plants?"

The Society has determined to repeat this queſtion, to which anſwers are to be ſent before November 1, 1803.

3. "The

— the aſcent of ſmoke.

3. "The Society demands a theory, or phyſical explication, which ſhall clearly and diſtinctly ſhow the cauſes of the aſcent of ſmoke in chimnies, or thoſe which prevent its riſing. Together with rules, deduced from this theory, for the conſtruction of chimnies, which ſhall point out the circumſtances neceſſary to be attended to in the ſeveral caſes to prevent ſmoke from entering the apartments?"

This queſtion is here again repeated, and the anſwer is demanded before the 1ſt of November, 1803, in order that the authors of memoirs may be enabled to correct them.

— dying plants.

4. "What indigenous plants, hitherto not uſed, may, from well confirmed experiments, afford good colours, which may be prepared and uſed with profit? And what exotic plants may be cultivated with profit upon the leſs fertile or uncultivated lands of the Republic, in order to the extraction of colours?"

The Society has determined to repeat this queſtion, which is continued without limit of time.

The queſtions for the preſent year are the following:

— effect of oxigen on colours.

5. "What do we learn, from the lateſt obſervations, reſpecting the influence of the oxigen of the atmoſphere, whether combined or not, together with the action of light upon the changes of colour? And what advantages may be derived from this knowledge?"

The Society deſires that it may be ſhown conciſely, and with preciſion, what is well proved by obſervations or experiments, in order that the actual ſtate of the ſcience with regard to this ſubject may be more readily apprehended, and greater advantages obtained in trade, or in the other branches of economy.

The term of concurrence is November 1, 1803.

— nouriſhment of plants.

6. "What light has been thrown upon the manner in which plants acquire their nouriſhment, in conſequence of the diſcoveries reſpecting the decompoſition of water and atmoſpheric air? And what deductions can be made from this knowledge for the improvement of uſeful vegetables?"

The term of concurrence is November 1, 1803.

7. "What facts are well proved with regard to the purification of corrupted water, and other impure ſubſtances, by charcoal? How far is it poſſible to explain this effect by the principles of chemiſtry? And what further advantages may be thence derived?"

The term of concurrence is November 1, 1803.

The following queſtions were propoſed formerly, but the term of concurrence is fixed to the firſt of November, 1803.

phyſiology of the human body;

1. "What light has the new chemiſtry thrown upon the phyſiology of the human body?"

diſeaſes;

2. "To what extent has this light been of uſe in rendering us better acquainted with the nature and cauſes of certain diſeaſes? And what uſeful conſequences, more or leſs grounded on experience, may be thence deduced for improving the practice of medicine?"

remedies;

3. "To what extent has the new chemiſtry been of uſe to afford preciſe notions concerning the action of certain external and internal remedies long in uſe or lately recommended? And what advantages may be obtained from a more exact knowledge in this reſpect in the treatment of certain diſorders?"

explanations.

As many philoſophers have mixed hypotheſes of ſlight foundation with the applications they have made of the principles of the new chemiſtry to the functions of the human body, which is no doubt very hurtful to the progreſs of ſcience, which might be ſo highly improved, if, according to the rule of Lavoiſier, nothing were adopted either in chemiſtry or the application of its principles, but what ſhall be founded on deciſive experiments; the Society deſires that thoſe who ſhall anſwer theſe queſtions ſhould preciſely diſtinguiſh facts from hypotheſes; and that with regard to the latter, they ſhould be merely pointed out, and their ſlight foundation briefly ſhown. For the principal aim of the Society in theſe queſtions, is to procure thoſe who exerciſe the practice of medicine and ſurgery in the Batavian Republic, and are not ſufficiently acquainted with the progreſs of the new chemiſtry and the application of its well eſtabliſhed principles to phyſiology, pathology, and theurapeutics, ſuch memoirs as may eaſily ſhow them what light the new chemiſtry has in effect thrown upon theſe ſciences; and alſo thoſe doctrines which, being of ſlight foundation, too haſtily adopted, or in themſelves doubtful, are not entitled to confidence.

Judgment will be given ſeparately upon the memoirs reſpecting each of theſe queſtions. They who propoſe to anſwer more than one, are therefore requeſted to do it ſeparately.

The above were propoſed in 1799.

Philoſophy of fire in the ma-

4. "What are the principles of the philoſophy of fire, reſpecting the production, the communication, and confinement

of

nagement of combuſtibles, &c.

of heat, which are neceſſary to be known in order to form a right judgment of the methods of uſing combuſtibles; and how can we, according to theſe principles, improve the fires for warming apartments and ſtoves for kitchen uſe, in order to ſave as much as poſſible the combuſtibles in uſe in this country?"

The above was propoſed in 1801.

ſtagnant water.

5. "What is actually known reſpecting the cauſes of the corruption of ſtagnant water, and is it poſſible to deduce from what is known or may be proved by deciſive experiments, what are the moſt effectual and ſafe means to prevent the corruption of ſtagnant waters?"

Prepared in 1801.

Prize, a medal of 30 ducats.

The prize for each queſtion is a gold medal of thirty ducats. The memoirs may be written in Dutch, French, or German; but only in Italic characters. They muſt be accompanied with a ſealed billet, containing the name and addreſs of the writer, and ſent to M. Van Marum, Secretary of the Society.

Fermentation.

The Batavian Society has propoſed as one of the prized ſubjects to be decided on the 1ſt of May 1802, the following queſtion: "What do we learn from the lateſt diſcoveries in chemiſtry reſpecting the nature of fermentation, and what advantages may reſult with regard to certain trades in which fermentative matters are uſed?" It was not thought fit to adjudge the prize; but without opening a new concurrence, the Society has engaged the author of a memoir, of which the device is, *Tout eſt important dans les voïes de la nature,* to ſupport his theory upon more deciſive experiments, and to extend its application to a greater number of manufactories; with promiſe of the crown if he ſhall accompliſh his purpoſe before the end of 1803 or 1804.

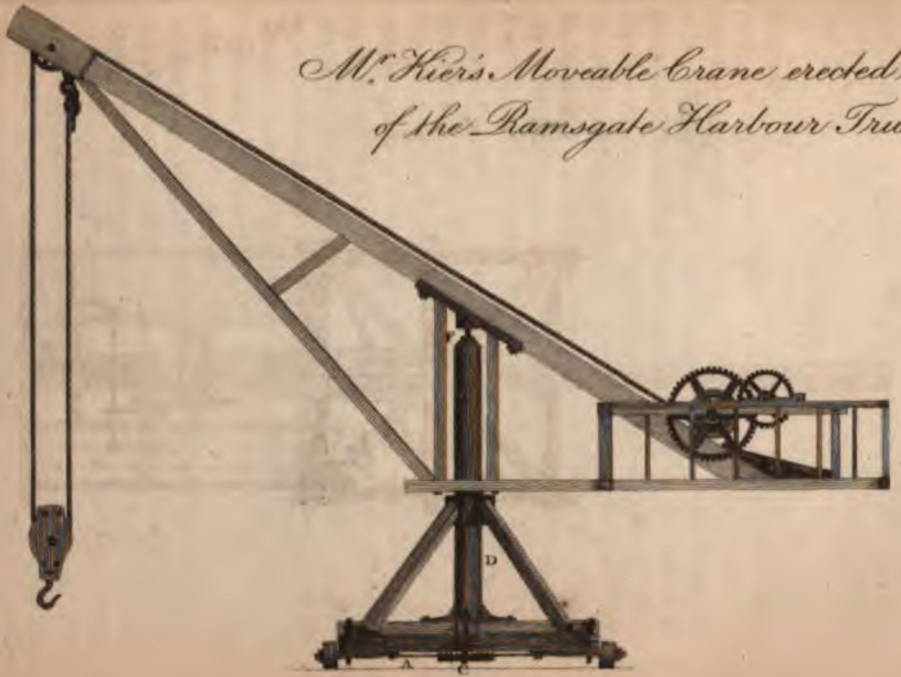
Sixth Deſcent of Garnerin with his Parachute.

Account of Garnerin's deſcent by a parachute.

On the 21ſt of September 1802, at ſeventeen minutes paſt five in the evening, the aeronaut Garnerin ſent off from the Parade in North Audley Street, an exploring balloon, which took a north-eaſt direction until it attained a very conſiderable elevation, and ſoon diſappeared. At fifty-eight minutes after five the great balloon itſelf aſcended with the parachute depending, and beneath it the cylindrical baſket in which Garnerin himſelf ſtood and repeatedly waved his flag. Its ſlow aſcent in the calmneſs of a beautiful evening, with the fixed and ſtill attention of the inhabitants of a great metropolis, to the amount of many hundreds of thouſands, produced a ſingular and very impreſſive effect on the mind. This pauſe of expectation which laſted nine minutes and a quarter, and carried the aeronaut ſo high as not to be perſonally diſtinguiſhed, was ſuddenly terminated by the ſeparation of the parachute from the balloon. It ſuddenly deſcended for near a ſecond, and then expanded. The balloon proceeded immediately to the ſouth, and was found the next day in Kent, about twelve miles out of town. Admiration and ſurprize now engaged the minds of all the ſpectators; which ſoon gave place to a marked ſentiment of terror from the extreme vibrations of the apparatus. The deſcent appeared ſlow and regular, but its vibrations (performed in about ſix ſeconds each) were ſo violent as frequently to carry the baſket nearly as high as the level of the parachute itſelf, which collapſed at its lower edge every time, ſo as to be nearly half-cloſed, and opened again when it reſumed its perpendicular ſituation. However firm the confidence of this daring adventurer may have been in the truth of his combinations, he muſt have have found himſelf in a ſituation requiring the exertions of all his courage. He deſcended in ſafety in a field near Pancras, having been very nearly ſix minutes in his deſcent. He ſeemed to have been upwards of a mile high, or probably about ſix thouſand feet; and on this ſuppoſition he fell at the rate of ſixteen feet in a ſecond. This is the velocity that would have been produced by jumping from the height of about four feet, and muſt therefore have been very ſafe.

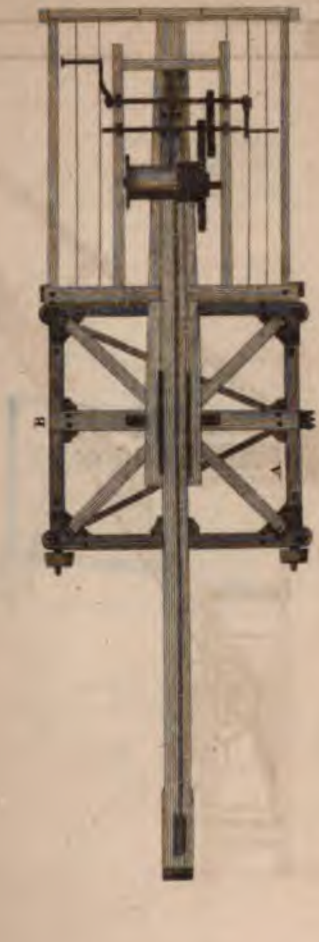
Velocity of his fall.

Mr. Kier's Moveable Crane erected by order of the Ramsgate Harbour Trust 1802.



Mutlow Sc. Russell Ct.

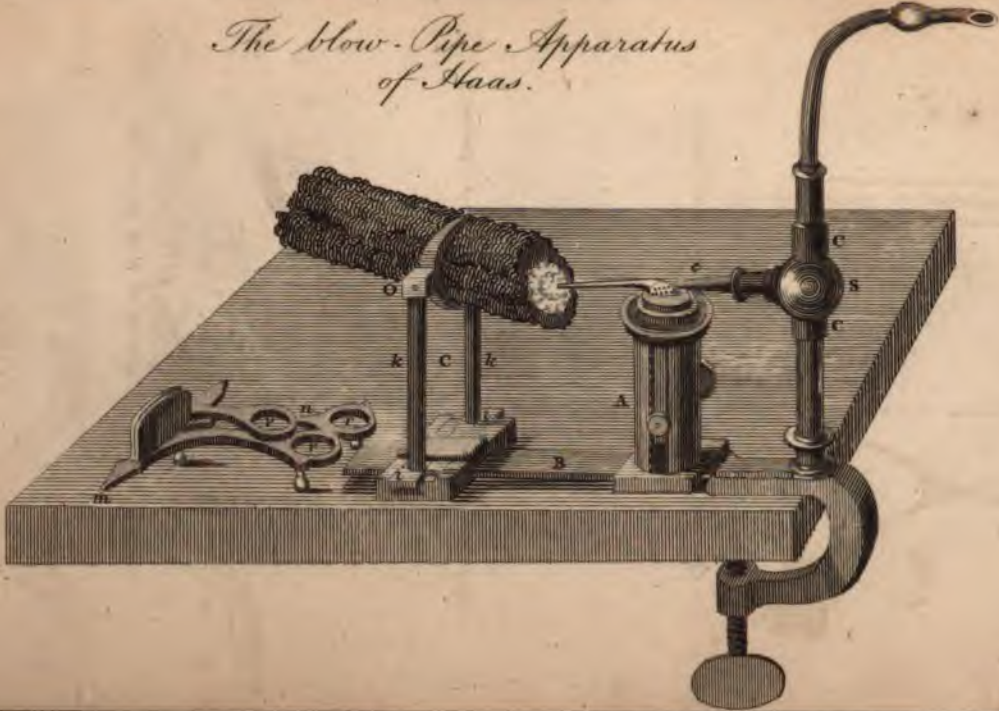
Philos. Journal Vol. III. Pl. VI. p. 244.



Plan of the Crane.

Mutlow Sc. Russell Ct.

The blow-Pipe Apparatus of Haas.



Mutlow Sc. Russell Cot.

B. Basalt on an uneven bottom.

Fig. 1.

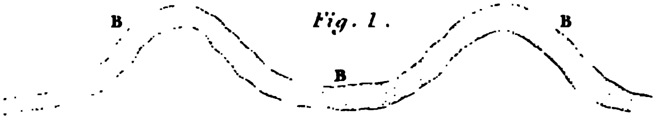


Fig. 2. B. Bed of Basalt broken by Water.

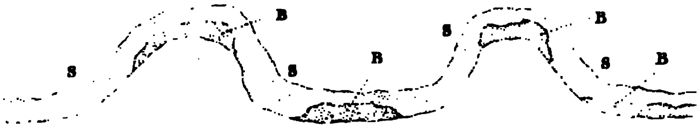
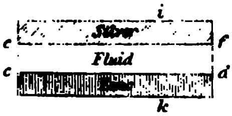


Fig. 2. S. Bed of Sandstone deposited on the broken bed of Basaltes.

Fig. 3.

Elements of the Galvanic Pile.



Distinguishers of Galvanism & Electricity.

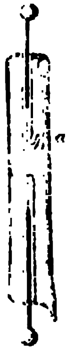


Fig. 4.

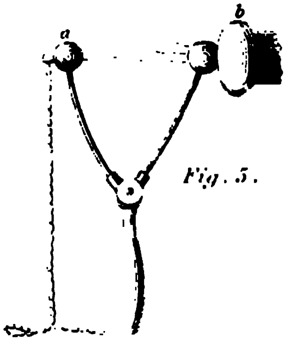


Fig. 5.

Mutlow sc. Russel Ct.

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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

NOVEMBER, 1802.

ARTICLE I.

Letter from Thomas Young, *M. D. F. R. S. In Reply to Mr.* Gough's *Letter, at Page* 36 *of the preſent Volume. On the Phenomena of Sound.*

To Mr. NICHOLSON.

SIR,

Further diſcuſſion apparently unneceſſary.

But Mr. Gough has not noticed the grave harmonics.

The facts,

I AM not certain that any anſwer, that I can make to Mr. Gough's reply to my letter, will add very materially to the elucidation of the ſubject in diſpute; a part of my object is already attained, for Mr. Gough ſeems to be no longer diſpoſed to contend for Dr. Smith's infallibility, and he appears to have formed in ſome meaſure a more complete idea of my original opinions, while he imagines that I have introduced ſome new modifications of them. I cannot however avoid remarking, that Mr. Gough has wholly omitted to notice the fundamental fact, which I ſtated as affording the moſt ſatisfactory of all proofs of the coaleſcence of ſounds; that is, the production of a faint, but very audible, graver ſound from the union of two acuter ones, a phenomenon ſo well know to muſicians, that I can ſcarcely ſuppoſe a perſon, who is ignorant of it, properly qualified to diſcuſs the ſubject of harmonics; and ſo important, with regard to the queſtion at preſent in diſpute, that I can ſcarcely conceive how a perſon, acquainted with it, can omit to endeavour to reconcile it with his opinions, eſpecially when challenged by his opponent to take it into conſideration. Here is a product of combination which poſſeſſes properties totally different from thoſe of its conſtituent parts; its pitch is always much lower, its quality of tone is perfectly ſingular, and the hearer is ſometimes almoſt inclined to imagine its direction to be different from that of the original ſounds; a circumſtance which he ſometimes expreſſes by ſaying, that the ſound "rings in his ears." Mr. Gough cannot controvert this fact by any reaſoning: if he can perſuade me that ſuch a ſound is a mixture and not a compound by partial coaleſcence, we ſhall then be nearly of the ſame opinion.

The grave harmonic is not a mixture, but a compound.

Illuſtration from a compound pendulum.

Will Mr. Gough deny, that when a ſmall pendulum is ſuſpended to the heavy weight of a larger one, its motion is compounded of larger and ſmaller vibrations? Or will he inſiſt that the reſult is a mixed motion, and not a compound one? The eye certainly follows each vibration ſeparately, although the vibrations may be ſo proportioned that a joint order and ſequence may be obſerved, which may produce effects peculiar to the combination, and diſcoverable in neither of the original motions. The compariſon appears to me to be perfectly appoſite, and if Mr. Gough allows it, I can ſcarcely imagine what cauſe there is left for further controverſy.

I beg leave once more to recommend the grave harmonics to Mr. Gough's notice, and to intreat him to ſuſpend any further reply to my remarks until he has made himſelf thoroughly acquainted with theſe intereſting phenomena.

I am, Sir,

Your faithful humble ſervant,

THOMAS YOUNG.

Royal Inſtitution,
Oct. 12, 1802.

II. On

II.

On the Galvanic Effect of very minute Particles of Zinc and Copper in Water. In a Letter from Mr. WILLIAM WILSON.

To Mr. NICHOLSON.

SIR, *London, October* 11, 1802.

I WAS lately grinding ſome ſmall plates of copper upon ſome plates of zinc (with fine emery), and after having waſhed them in a cup of water to cleanſe them, I left them about five hours. When I returned I found the water in the cup covered with a dirty froth, and upon taking up the cup to examine it, the agitation cauſed innumerable bubbles of air to riſe from the ſediment at the bottom of the water. By continuing to agitate the water for ſeveral minutes, they decreaſed in quantity, and at laſt diſappeared. I now left it about four or five hours, and then examined it again, when the bubbles were nearly as copious as ever, and, with nearly the ſame quantity of agitation, again ceaſed to be produced.

Copper and zinc were ground with emery. The mud after ſubſidence emitted gas; and at remote intervals.

I examined this water every four or five hours for five days, and the appearance was the ſame, though weaker and weaker. After the fifth day I could not find that any bubbles were generated; I therefore (ſuppoſing this appearance to be occaſioned by the particles of copper and zinc forming a kind of galvanic ſeries) took equal quantities of filings of copper and zinc and put them into a glaſs of water, and ſtirred it about to mix them as much as poſſible. In about three hours afterwards there were ſome bubbles of air adhering to the filings, which by agitation roſe to the top of the water; and the ſame phenomenon was repeatedly exhibited. In ſhort theſe metallic particles acted preciſely ſimilar to the former, with only this difference: It required more violent agitation; the air bubbles were larger, and the property diſappeared much ſooner, for there were no bubbles generated after the third day. This difference I ſuppoſed to be owing to the difference in the ſize of the particles of the metals; for in the firſt caſe the particles, by reaſon of their ſmallneſs, would form a more numerous ſeries and extenſive ſurface of contact, than the filings could in the latter.

The effect laſted five days.

Copper and zinc filings were tried, with the ſame effect.

Explanation of the difference.

The impalpable powder of the metals.

Laſtly, I repeated the firſt experiment by waſhing ſome emery very fine, and ſlightly ground with it a plate of zinc on another of copper, and then waſhed them in a glaſs of clear water. It was ſeveral hours before the ſediment had all fallen to the bottom of the glaſs; but when it had, it acted nearly with the ſame power as in the firſt caſe, and continued to act quite as long *.

If, Sir, you ſhould judge the above account worthy of a place in your Philoſophical Journal, I ſhall conſider myſelf very much obliged by its inſertion.

I am, Sir,
Your obedient humble ſervant,
Wm. WILSON.

III.

On the Colours obtained from Metallic Oxides, and fixed by Means of Fuſion on different Vitreous Bodies. By Alex. Brongniart, *Director of the National Manufactory of Porcelain at Sévres, Engineer of Mines, &c.*

(Concluded from Page 101.)

Concerning the Red, Roſe, and Brown Colours obtained from Iron.

Red colours from the oxide of iron.

THESE colours are made from red oxidated iron prepared with nitric acid. The oxides are calcined ſtill more by expoſing them to the action of fire. If too much heated, they change to a brown.

Compoſition.

Their flux is compoſed of borax, ſand, and minium in ſmall quantity.

They may be ſubſtituted for the oxide of gold.

Theſe are the oxides which afford the roſe and red colours that may be ſubſtituted inſtead of the ſame colours made from oxide of gold. If properly applied on hard porcelain, they never change. I have made roſes with theſe colours, and there was no difference between the flower before and after the baking, except that brilliancy which colours naturally receive by fuſion.

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* Theſe galvanic facts appear to conſtitute one of the caſes detailed with larger pieces of metal, and obſcurely explained by Fabbroni in his curious paper in the Philoſ. Journal, quarto ſeries, IV. 120. —W. N.

The colours may either be previouſly fuſed or not at pleaſure.

How affected by violent heat.

In a violent fire they either partly diſappear, or produce a dull and brick-duſt red colour, which is not at all agreeable.

They are good for glaſs and tender porcelain;

Their compoſition is the ſame either for tender porcelain or for glaſs. They do not change on the latter, but on the former they almoſt intirely diſappear by the firſt fire, and they muſt be laid on very heavily in order to have any part viſible.

but muſt be heavily laid on the latter.

It is to the preſence of lead in their glaze that this ſingular effect muſt be attributed. I have aſcertained this by a very ſimple experiment. I placed this colour on window-glaſs and fired it very ſtrongly, and it did not change.

Experiments;

I then covered ſome parts of it with minium, and again expoſed it to the fire. The colour totally diſappeared in thoſe places where the red oxide of lead had been applied.

When I performed this operation on a larger ſcale in cloſed veſſels, a large quantity of oxigen gas was diſengaged.

which ſhew that oxide of lead *diſcolours* glaſs.

This obſervation I think clearly proves the effect of oxided lead as a diſcolourer of glaſs: we ſee that it does not operate, as has been ſuppoſed, by burning combuſtible impurities in the glaſs, but by diſſolving, diſcolouring, or volatilizing the oxide of iron which may affect its clearneſs.

Concerning the Yellows.

Yellows are formed by the oxides of lead and antimony;

Yellows are colours that require much precaution in fabricating, on account of the lead they contain; which ſometimes, by approaching to the metallic ſtate, produces black ſpots.

The yellows of hard and tender porcelain are the ſame. They are compoſed of oxide of lead, white oxide of antimony, and ſand.

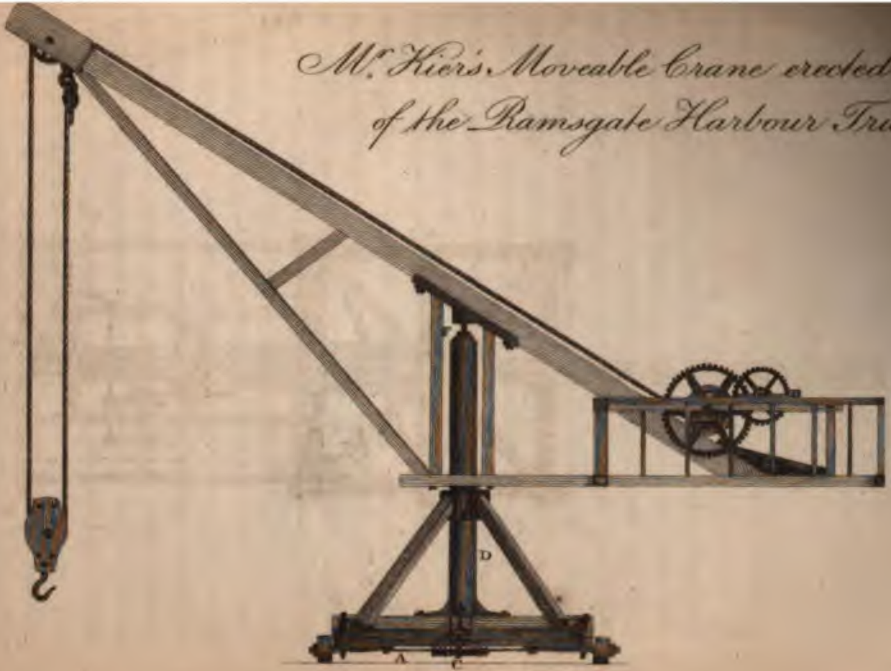
and ſometimes tin.

Oxide of tin is ſometimes added; and when it is required very lively and reſembling the colour of the marigold, red oxide of iron is added, the very deep colour of which diſappears during the previous fuſion they undergo, on account of the lead contained in this yellow. When theſe colours are once made, they do not change; they diſappear almoſt intirely in the porcelain fire,

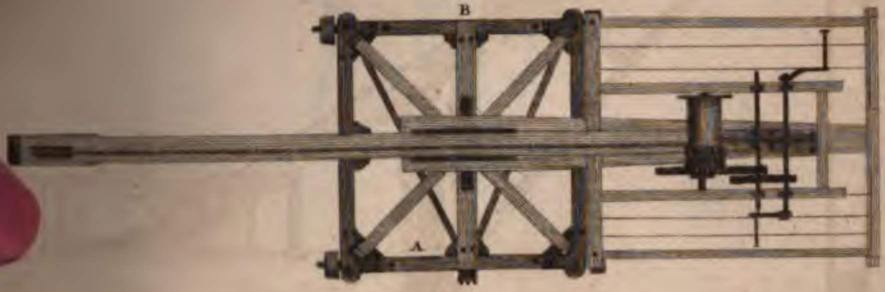
They are not changeable.

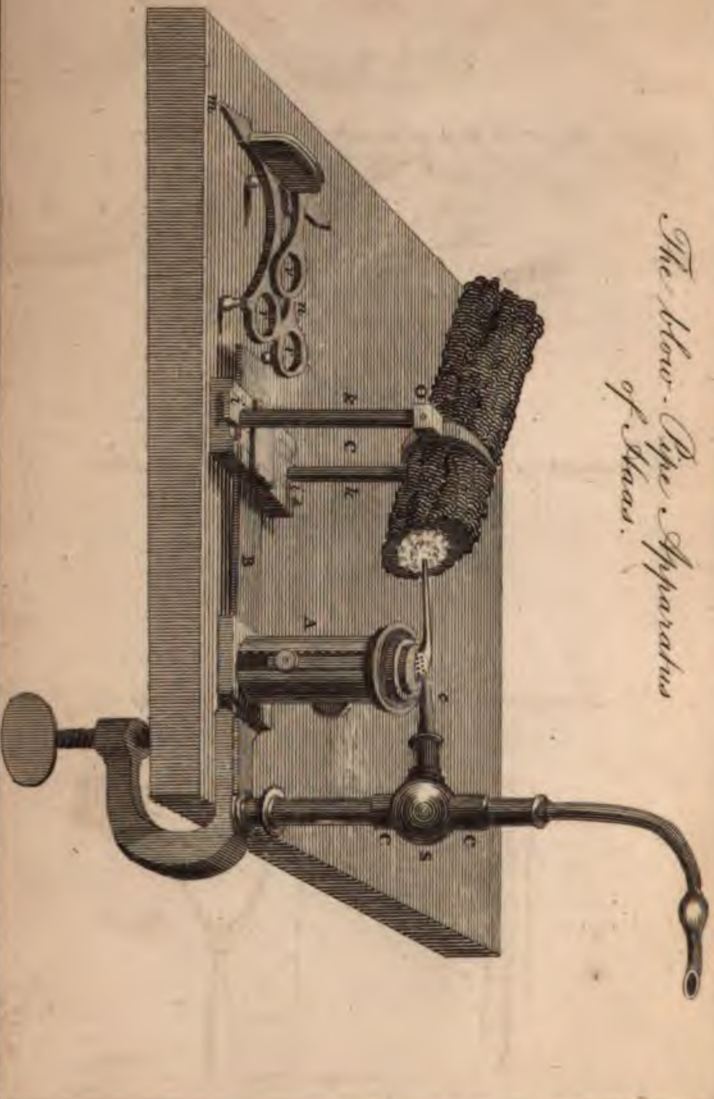
Theſe

Mr. Hier's Moveable Crane erected by orde
of the Ramsgate Harbour Trust 1802.



Plan of the Crane.





The Blow-Pipe Apparatus of Haas.

Mutlow Sc. Ruſsell Ct.

B. Basalt on an uneven bottom.

Fig. 1.

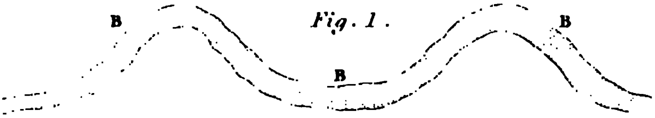


Fig. 2. B. Bed of Basalt broken by Water.

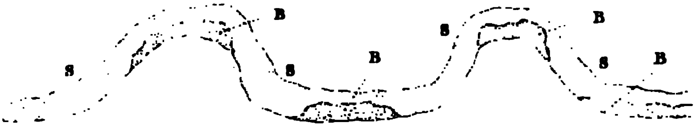
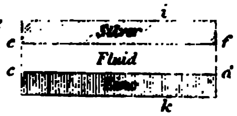


Fig. 2. S. Bed of Sandstone deposited on the broken bed of Basaltes.

Fig. 3.



Elements of the Galvanic Pile.

Distinguishers of Galvanism & Electricity.

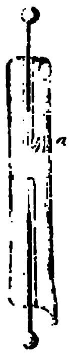


Fig. 4.

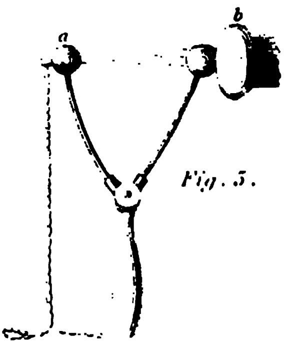


Fig. 5.

and have conſtantly found it to be malleable. The ſame remark may be applied to the native iron from South America; and alſo to that from Senegal.

Another ſpecimen; more compact,

The ſecond of the two ſpecimens mentioned above, and which weighs ſeveral pounds, preſents an aſpect that differs, in ſome reſpects, from that of the preceding ſpecimen. The moſt conſiderable part of it forms a ſolid compact maſs, in which there is not to be perceived the ſmalleſt appearance of pores or cavities; but there ariſes upon its ſurface, a kind of ramified or cellular part, ſimilar, in every reſpect, to the ſpecimen already deſcribed, and every where completely connected with the ſubſtance of the maſs itſelf.

conſiſting of iron and the greeniſh tranſparent matter, which fills its cavities.

If the compact part of this piece is examined with attention, it will be perceived, that it is not entirely compoſed of iron in the metallic ſtate, but that it is mixed with nearly an equal quantity of the tranſparent ſubſtance of a yellowiſh green colour, (ſometimes alſo of a greeniſh yellow,) already ſpoken of in the deſcription of the other ſpecimen. This ſubſtance is mixed with the iron, in ſuch a manner, that if the whole of the former could be removed, the remaining part would conſiſt merely of iron in the metallic ſtate, and would preſent the ſame cellular appearance as the preceding ſpecimen, and the ramified or cellular part of the ſpecimen now deſcribed.

The green ſtony part looks like glaſs:

This ſtony part, ſeparated from the iron, appears in the form of ſmall nodules, generally of an irregular ſhape, but ſometimes nearly globular: they have a perfectly ſmooth and ſhining ſurface, ſo as very often to preſent the appearance of ſmall balls of glaſs; a circumſtance that has led many perſons to ſuppoſe them the reſult of a real vitrification. Some of theſe nodules have ſeveral irregular facets, produced by the compreſſion of the iron in which they were incloſed; but I have never obſerved in them, any appearances that could lead me to ſuſpect they had the ſlighteſt tendency whatever to aſſume a determined cryſtalline form.

is always tranſparent; cuts glaſs, but not quartz; fracture conchoidal; electric by friction;

This ſubſtance is always more or leſs tranſparent. It is ſufficiently hard to cut glaſs; but has no effect upon quartz. It is very brittle: its fracture is uſually conchoid; but I could not perceive that it broke in any particular direction, in ſuch a way that I could conſider the fracture as a natural one. It becomes electric by friction. Its ſpecific gravity is from 3263 to 3300. It is very refractory: I kept it, for ſome time, expoſed to a degree of heat ſufficiently ſtrong to oxidize, to a conſiderable depth, the iron crucible in which it was placed, without its having undergone any alteration, except that of having acquired a greater degree of intenſity in its colour. Its tranſparency was not at all diminiſhed. I think, therefore, there is not the ſmalleſt reaſon to allow any probability to the opinion that it ought to be conſidered as a kind of glaſs.

not fuſible in an iron veſſel.

Of all ſubſtances hitherto known, that with which it ſeems to have the greateſt analogy, is the peridot, (the chryſolite of Werner,) to which ſome mineralogiſts have referred it. The reſult of Mr. Howard's analyſis of it, is nearly the ſame as that of the analyſis of the peridot, made by Mr. Klaproth.

It reſembles the peridot;

The hardneſs and infuſibility of this ſubſtance are nearly the ſame as thoſe of the peridot; but it ſeems to have a rather leſs degree of ſpecific gravity: that of two very perfect cryſtals of peridot, I found to be from 3340 to 3375. The cryſtalline forms of the ſubſtance here deſcribed, if ever we ſhould be able to determine them, would clear up our doubts reſpecting the analogy between the two ſubſtances. If we conſider the compact part of the ſpecimen now treated of, particularly the ſtrong connection that appears to exiſt between the iron and the tranſparent ſubſtance, and the great reſiſtance we experience when we attempt to ſeparate them, we cannot help being ſurpriſed, that almoſt all the ſpecimens of this maſs of metallic iron that have been brought to Europe, are in the cellular ſtate already deſcribed, owing apparently to the total, or almoſt total, deſtruction of the tranſparent ſubſtance. But, beſides the fragility of this ſubſtance, the ſpecimen in queſtion helps very much to explain the above cirumſtance, inaſmuch as many of the nodules of the tranſparent ſubſtance belonging to it, are in a ſtate of real decompoſition. In that ſtate, they are changed into a white opaque ſubſtance, which, upon being lightly preſſed or ſqueezed between the fingers, crumbles into a gritty dry powder. This decompoſition may be obſerved to have taken place in various degrees: in many of the nodules, the ſubſtance is merely become friable, without being much altered in its appearance; whereas, ſome of thoſe which are in a ſtate of complete decompoſition, are of an ochreous reddiſh yellow colour; it is, however, eaſy to diſtinguiſh that this colour does not belong to them, but is owing only to the oxiidizement of the adjacent particles of iron.

in hardneſs and infuſibility.

It is curious that it ſhould be deſtructible by the weather;

but it undoubtedly is ſo.

This fact explains the ſtructure of native iron, &c.

From the above obſervations, it will not be difficult to conceive the poſſibility of the total, or nearly total, deſtruction of the tranſparent ſubſtance; and alſo, the appearance the pieces of iron muſt naturally preſent, when deprived of it. I cannot help obſerving likewiſe, that there appears to exiſt a very intereſting analogy, between theſe tranſparent nodules and the globules I deſcribed as making part of the ſtones ſaid to have fallen on the earth. This analogy, though not a very ſtrong one, may lead us to ſuppoſe that the two ſubſtances are ſimilar in their nature, but that the globules are leſs pure, and contain a greater quantity of iron.

The Bohemian iron in ſome meaſure reſembles the Siberian.

The native iron from Bohemia is a compact maſs, ſimilar to the compact part of the large ſpecimen of iron from Siberia, which has juſt been deſcribed: like that, alſo, it contains a number of globular bodies or nodules; but they are not in ſuch great proportion as in the Siberian iron. They are beſides perfectly opaque, and very much reſemble the moſt compact of the globules belonging to the ſtones ſaid to have fallen on the earth.

EXAMINATION OF THE IRON FROM SOUTH AMERICA.

S. American iron contains 10 per cent. of nickel.

I have already obſerved, that my experiments coincided with thoſe of Mr. Prouſt. He obtained 50 grains of ſulphate of nickel, from 100 of this maſs. The proceſs I have ſo frequently mentioned, yielded me 80 grains of oxide of iron from 62 of the metal; which indicates about 7½ of nickel, or about 10 per cent.

EXAMINATION OF THE SIBERIAN IRON.

Siberian iron 17 per cent. &c.

100 grains of this iron, gave 127 of oxide of iron: hence, it ſhould contain about 17 per cent. of nickel.

The yellow matter.

The yellow ſubſtance belonging to this iron, was analyzed in the ſame way as the globular bodies, and the earthy parts, of the ſtone from Benares.

The proportions, reſulting from the analyſis of 50 grains, and from ſome previous experiments on other particles, were,

Silica

Silica - - - -	27
Magneſia - - - -	13½
Oxide of iron - - - -	8½
Oxide of nickel - - -	½
	49½

EXAMINATION OF THE BOHEMIAN IRON.

26½ grains of this metal, left about 1½ grain of earthy matter, inſoluble in nitric acid; and, by ammonia, afforded 30 grains of oxide of iron, inducing an eſtimation of nearly 5 of nickel.

Bohemian iron 17 per cent. do.

EXAMINATION OF IRON FROM SENEGAL, BROUGHT BY GENERAL O'HARA, AND GIVEN TO ME BY MR. HATCHETT.

In this experiment, 199 grains of oxide were produced from 145 grains of metal; hence, there may be an eſtimation of 8 grains in 145, or between 5 and 6 per cent. of nickel.

Senegal iron 5 or 6 per cent. ditto.

It will appear, from a collected view of the preceding pages and authorities, that a number of ſtones aſſerted to have fallen under ſimilar circumſtances, have preciſely the ſame characters. The ſtones from Benares, the ſtone from Yorkſhire, that from Sienna, and a fragment of one from Bohemia, have a relation to each other not to be queſtioned.

Summary of the characters of the ſtones fallen on the earth.

1ſt. They have all pyrites of a peculiar character.

2dly. They have all a coating of black oxide of iron.

3dly. They all contain an alloy of iron and nickel. And,

4thly. The earths which ſerve to them as a ſort of connecting medium, correſpond in their nature, and nearly in their proportions.

Pyrites, oxide of iron, nickel, earths correſpondent.

Moreover, in the ſtones from Benares, pyrites and globular bodies are exceedingly diſtinct. In the others they are more or leſs definite; and that from Sienna had one of its globules tranſparent. Meteors, or lightning, attended the deſcent of the ſtones at Benares, and at Sienna. Such coincidence of circumſtances, and the unqueſtionable authorities I have adduced, muſt, I imagine, remove all doubt as to the deſcent of theſe ſtony ſubſtances; for, to diſbelieve on the mere ground of incomprehenſibility, would be to diſpute moſt of the works of nature.

Meteors or lightning accompanying them.

Native irons.

Reſpecting the kinds of iron called native, they all contain nickel. The maſs in South America is hollow, has concavities, and appears to have been in a ſoft or welding ſtate, becauſe it has received various impreſſions.

The Siberian iron has globular concavities, in part filled with a tranſparent ſubſtance, which, the proportional quantity of oxide of iron excepted, has nearly the compoſition of the globules in the ſtone from Benares.

The iron from Bohemia adheres to earthy matter ſtudded with globular bodies.

The Senegal iron had been completely mutilated before it came under my examination.

From theſe facts, I ſhall draw no concluſion, but ſubmit the following queries.

Have theſe all the ſame origin? and from meteors?

1ſt. Have not all fallen ſtones, and what are called native irons, the ſame origin?

2dly. Are all, or any, the produce or the bodies of meteors?

And, laſtly, Might not the ſtone from Yorkſhire have formed a meteor in regions too elevated to be diſcovered?

Specimens of the Benares and Yorkſhire ſtones have been depoſited, by the Preſident, in the Britiſh Muſeum.

V.

Experiments and Obſervations on the Heat and Cold produced by the Mechanical Condenſation and Rarefaction of Air[*]. *By* JOHN DALTON.

Well known facts that the thermometer riſes in condenſed and falls in rarefied air.

IF a thermometer be incloſed in a receiver and the air ſuddenly condenſed, the thermometer riſes a few degrees above the temperature of the atmoſphere; and if the air be exhauſted from a receiver incloſing a thermometer, the mercury ſinks a few degrees immediately; but in both caſes after ſome time it reſumes its former ſtation. Theſe facts are well known to philoſophers of the preſent age, but they do not all agree in the explanation of them. Thinking the ſubject worthy of elucidation, I was induced to inſtitute a ſeries of experiments for the purpoſe, which I apprehend have led to a clear demonſtration of the cauſe of the phenomena, and moreover makes the facts themſelves appear in a ſomewhat different point of view from what they are ſeen in at the firſt moment.

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[*] Mancheſter Memoirs, V. 515.

One circumſtance is very remarkable, that whether the mercury riſes or falls in theſe inſtances, it is done *very rapidly*; whereas in the open air, if a thermometer be only two or three degrees above or below the temperature, it moves very ſlowly. This ſeems to have ſuggeſted to every one the idea, that the elaſticity of the glaſs bulb of the thermometer has a principal ſhare in producing the effect, by cauſing the bulb to yield a little to the preſſure of the air. It has however been found upon trial that the ſame effects take place whether the thermometer is ſealed or not. My experiments accord with this, having made a thermometer and left it unſealed for the expreſs purpoſe; in all the experiments with condenſed and rarefied air, there was no ſenſible difference obſerved to ariſe from the inequality of preſſure on the external and internal ſurfaces of the bulbs, the ſealed and open thermometers varying the ſame in kind and alſo in degree, except from circumſtances to be noticed hereafter.

This riſe and fall are *very rapid*, whence it was ſuppoſed to ariſe from preſſure on the glaſs bulb; but it happens whether the tube be ſealed or not.

It being certain then that a real change of temperature takes place, it remained to determine the quantity and manner of that change. Having choſen a ſmall and conſequently ſenſible thermometer, with a ſcale of degrees ſufficiently large to admit of diſtinguiſhing one tenth of a degree, I proceeded to aſcertain ſeveral facts experimentally.

Conſeq. a real change of temperature takes place.

EXPERIMENT 1.

Took a receiver, the capacity of which was about 120 cubic inches, and ſuſpended the thermometer with its clear bulb in the central part of it; then letting the whole acquire the temperature of the room, which was without a fire, I exhauſted the air and afterwards reſtored it, marking the effects upon the thermometer. The medium of ſeveral trials nearly agreeing with each other was as under:

Exp. I. A very ſmall thermometer in a ſmall receiver fell by exhauſtion, and the contrary *very ſuddenly*.

The thermometer in the air of the room ſtood at -	36°.8
———— ſunk upon exhauſtion to - - -	34 .7
———— roſe when the air was reſtored to	38 .9

The *ſuddenneſs* of the fall and riſe puzzled me moſt: after reflecting upon it for ſome time, I conjectured that the real change of temperature of the air or medium was much greater whence it was inferred, that the real change of temperature was very great; but not very effective becauſe ſo momentary.

than the thermometer indicated, but that the inequality exiſted only for a few ſeconds of time, becauſe the receiver, &c. immediately impart heat to or abſtract it from ſo ſmall a quantity of air as 120 cubic inches, which are only equal to 40 grains in weight. The phenomena of the thermometer ſeemed very well to accord with the ſuppoſition of *great heat or cold* acting upon it for a few ſeconds only.

EXPERIMENT 2.

Exp. 2. A large and a ſmall thermometer were cooled differently.

Purſuing this idea, I imagined that if two thermometers whoſe bulbs were very unequal in magnitude were incloſed together, the ſmaller bulb ought to give the greater variation: accordingly I incloſed two, the diameters of their bulbs being .35 and .65 of an inch reſpectively; and having exhauſted the air and reſtored it again repeatedly in ſucceſſion, and found a mean of the variations, that of the ſmall bulb was 2°.8, and that of the large, 2°.2.

EXPERIMENT 3.

Exp. 3. And the ſame therm. was more cooled the more remote from conductors of heat.

Repeated the exhauſtion with the ſmall thermometer incloſed in three different circumſtances ſucceſſively; 1ſt with the bulb in the centre of the receiver; 2d with the bulb reſting on the wet leather of the plate; and 3d with the bulb reſting againſt the ſide of the receiver.

1ſt Caſe—Reduced by exhauſtion - -	2°.45
2d Caſe - - - - - -	2 .15
3d Caſe - - - - - -	1 .2
1ſt Caſe—Raiſed by reſtoring the air - -	4 .05
2d Caſe - - - - - -	2 .25
3d Caſe - - - - - -	2 .8

EXPERIMENT 4.

Exp. 4. A cubic inch of water was ſenſibly cooled.

Incloſed a wine glaſs with about a cubic inch of water in it, containing the bulb of a thermometer, in a receiver; and, exhauſting the air, the thermometer ſunk half a degree ſuddenly, and then continued ſtationary: upon reſtoring the air it ſuddenly roſe half a degree.

From all which it follows that the real change was much

All theſe experiments confirmed my conjecture of a much greater degree of heat and cold being produced in theſe caſes than the thermometer points out, but that its continuance is ſo ſhort as not to effect a material change in the temperature of the mercury. The following experiments were made to aſcertain what may be the *real* degree of heat and cold generated in thoſe operations.

greater than ſhewn by the thermometer.

EXPERIMENT 5.

The ſame receiver and ſmall thermometer as above being uſed, I found the exhauſtion was effected by working the pump *one minute*. The thermometer ſunk nearly 2° in the firſt half minute, and the remainder, a few tenths of a degree, in the latter half minute. The operation being ſtopped, and things remaining in the ſame ſtate, it required ſome minutes of time before the thermometer recovered *one* degree of the heat loſt. Upon opening the cock, the receiver filled with air in five ſeconds, and the greateſt velocity of the riſing mercury was about the end of that time. The riſing continued for 30 or 40 ſeconds from its commencement, but ¾ of the effect were produced in the firſt 10 ſeconds. The greateſt velocity of the riſing mercury is 1° in 3½ ſeconds. After the thermometer had attained its utmoſt height, it began to fall again at the rate of $\frac{1}{10}$ of a degree in a minute.

Exp. 5. The time of ſubſidence much leſs than of ſubſequent riſe in the vacuum; and the time of riſe by the introduction of the external air much leſs than of the ſubſequent fall.

EXPERIMENT 6.

Took the ſame thermometer and heated it to 50° above the temperature of the air, then let it be cooled by the medium of air, and it began to fall at the rate of 1° in 3½ ſeconds.

The two laſt experiments ſeem to prove that when air is let in to the receiver in the ordinary way, *an increaſe of temperature of 50° is produced in the medium within the receiver for 3½ ſeconds*. This high temperature is reduced *in a few ſeconds* by the receiver and ſurrounding bodies, to their own temperature.

Exp. 6. By heating the thermometer and letting it cool in the air, it was found that an elevation of 50° was required to produce as rapid an effect as in the former experiment. Such therefore was the real riſe,

EXPERIMENT 7.

On condenſed Air.

Took a large ſpherical glaſs receiver, the capacity of which was ſomething more than twice that of the former (above one gallon), and ſuſpended a thermometer in the centre of it, of a larger bulb than that before uſed; the receiver had a braſs cap and ſtop-cock adapted to it: then doubled the denſity of the air within it by a condenſer. The thermometer roſe 2° or more. Let out the air ſuddenly and the thermometer imme-

Exp. 7. Thermometer in condenſed air roſe ſuddenly when the air was let in.

diately ſunk each time from 3° to 3°.5; at the ſame time an exceedingly denſe miſt was produced in the receiver, which ſoon ſubſided.

Suſpecting that *aqueous vapour,* which always exiſts in the atmoſphere, and is liable to aſſume the liquid or aërial form according to circumſtances, might be the principal agent in the production of heat and cold by condenſation and rarefaction, I thought that an increaſe of it might produce a greater effect, and that cold air, which contains leſs vapour, might have a leſs effect. The reverſe however was the fact, as appears by the following,

EXPERIMENTS 8 and 9.

Exp. 8 and 9. Theſe effects did not ariſe from the water ſuſpended in the air.

For the change was leſs when the vapours were moſt abundant.

In a cold morning laſt winter when the air was clear and the thermometer without ſtood at 20°, I took the receiver and condenſer into the open air, and let them ſtand for 15 minutes to acquire its temperature; then repeatedly condenſed the air to a double denſity, and ſuddenly liberated it again. On a medium of five trials the mercury fell 3°.3 on opening the cock. The vapour precipitated was whiter than uſual and not nearly ſo denſe.

Again, took the receiver and condenſer into a dyer's ſtove where the temperature was about 100°, and the air abounded with vapour in a tranſparent ſtate: after ſome time, condenſed the air and liberated it as before, when on a medium of five trials the mercury ſunk only 3°, and a very copious miſt was precipitated, ſo denſe that one could but juſt diſtinguiſh the degree of the thermometer through it.

This might be inferred from the known theory.

Theſe experiments ſhew that the greater the quantity of vapour condenſed the leſs is the change of temperature; and that conſequently, if air was entirely free from vapour, the change of temperature would be a *maximum.* Indeed this is clearly conſiſtent with the known law, that when vapour is condenſed, heat is given out. Any proceſs to cool the air muſt be retarded by the condenſation of part of the vapour it contains. Suppoſe for inſtance that a portion of the atmoſphere contained $\frac{1}{60}$ of its weight of aqueous vapour, and that $\frac{3}{5}$ of this vapour were condenſed by 50° of cold; that is, $\frac{1}{100}$ of the whole elaſtic maſs was converted into water; then the heat given out would be ſufficient to raiſe the temperature of the remaining maſs of air and vapour 6 or 8°, which ſufficiently accounts for the ſmall difference obſerved in the reſults upon warm vapoury air and cold dry air. Hence vapour, far from producing the change of temperature in queſtion, tends to diminiſh the effect.

If any doubt remained with me reſpecting the *real* change of temperature that takes place in the operations related above, it was completely removed by the reſults of the two following experiments.

EXPERIMENT 10.

Incloſed a ſmall graduated glaſs tube of $\frac{1}{15}$ of an inch internal diameter, and 10 inches long, with a ſhort column of mercury in it, in the large receiver; the tube was ſealed at one end and open at the other, ſo that a portion of given capacity was confined by the mercurial column, which was near the open end of the tube, and ſubject to riſe or fall by any variation of elaſticity of the air on either ſide, being a proper manometer: then doubled the denſity of the air in the receiver, and opening the ſtop-cock, the mercurial column ſoon ran up to its former ſtation, but inſtantly turning the cock again, the mercurial column returned or fell down gradually for 5 or 10 ſeconds, to the amount of nearly $\frac{1}{10}$ of the whole aërial column, and then became ſtationary. Again opening the cock, a quantity of air ruſhed out, and the mercury reſumed its original ſtation. Theſe effects were always the ſame, on a repetition of the experiment.

A ſmall manometer was incloſed;

the denſity of the ſurrounding air was doubled; then reſtored by a momentary communication from without.

The manometer was affected as if the air in the receiver had gradually abſorbed heat and become more elaſtic.

EXPERIMENT 11.

Let the mercurial column of the manometer down by a wire to $\frac{1}{4}$ of the length of the tube from the ſealed end; then exhauſted $\frac{3}{4}$ of the air from the receiver, which was ſeen by the mercury riſing to the top of the tube; and upon opening the cock the mercury fell to its former ſtation, but then ſuddenly turning the cock, the mercury gradually roſe for the ſpace of 5 or 10 ſeconds to *more* than $\frac{1}{10}$ of its original height above the ſtationary point, and remained there till the cock was opened: after which it reſumed its proper ſtation.

Exp. 11. The manometer as before, but in rarefied air:

The air let in ſhowed ſigns of giving out heat.

The phenomena in the two laſt experiments can be explained only on the following principle:—The air in the receiver and in the manometer is ſubject to a like degree of rarefaction and *condenſation in thoſe* experiments, or nearly ſo.

Fuller explanation. The air in the manometer recovers the common temperature ſooner than that in the receiver: whence in the former caſe the gradual heating of the air in the receiver renders its preſſure from elaſticity greater on the mercury: and the contrary in the other caſe.

ſo. When the equilibrium of heat in the air is diſturbed by the operations of condenſation and rarefaction, it is reſtored in the manometer *inſtantly* by reaſon of the contiguity of the glaſs to the air; but in the large receiver it requires a ſenſible time of 10 ſeconds or more to reſtore the equilibrium throughout the whole internal capacity. It is this reſtoration that increaſes or diminiſhes the elaſticity of the air confined in the receiver, and thereby cauſes the retrogradation of the mercurial column. Now I have found by former experiments, that a change of 50° in temperature effects a change of $\frac{1}{10}$ nearly, in the capacity or bulk of air. It follows therefore that in the caſe of reſtoring the equilibrium in condenſed air, about 50° of cold is propuced; and in letting in air to an exhauſted receiver ſomething more than 50° of heat is produced. The ſmall difference ſeems to ariſe from this, that the condenſation of vapour in the former caſe *diminiſhes* the effect, and in the latter, if any there be, *increaſes* the effect, that would ariſe from operating upon purely dry air.

The theory of theſe effects reſts on the difference of capacity of air for heat; viz. *leſs* the *greater the denſity.*

The experiments and obſervations hitherto related go principally to aſcertain facts without any reference to the theory of them: This however may be given in a few words, and is the ſame that is aſcribed to Mr. Lambert by Meſſrs. Sauſſure and Pictet and by them adopted. He conceives that a vacuum has its proper capacity for heat, the ſame as air, or any other ſubſtance; and that the capacity of a vacuum for heat is greater than that of an equal volume of atmoſpherical air; alſo that the *denſer* air is, the *leſs* is its capacity for heat: upon theſe principles the phenomena are eaſily referable to that claſs of chemical facts where heat and cold are generated by the mixture of two different bodies. If this theory be right,

Whence new experiments may be inſtituted on the capacities of elaſtic fluids.

and I think there is little doubt of it, we may hence be led into a train of experiments, by which the abſolute capacity of a vacuum for heat may be determined; and likewiſe the capacities of the different gaſes for heat, by a method wholly new: but this muſt be left to future inveſtigation.

VI. An

VI.

An Account of a Method of copying Paintings upon Glaſs, and of making Profiles by the Agency of Light upon Nitrate of Silver. Invented by T. WEDGWOOD, *Eſq. With Obſervations by* H. DAVY. *From the Journals of the Royal Inſtitution,* I. 170.

White paper, &c. wetted wit nitrate of ſilver, becomes black b light:

WHITE paper, or white leather, moiſtened with ſolution of nitrate of ſilver, undergoes no change when kept in a dark place; but, on being expoſed to the day-light it ſpeedily changes colour, and, after paſſing through different ſhades of grey and brown, becomes at length nearly black.

with different ſpeed as the ligh is ſtronger; *e. g* 2 minutes in ſunſhine.

The alterations of colour take place more ſpeedily in proportion as the light is more intenſe. In the direct beams of the ſun, two or three minutes are ſufficient to produce the full effect. In the ſhade ſeveral hours are required, and light tranſmitted through different coloured glaſſes, acts upon it with different degrees of intenſity. Thus it is found that red rays, or the common ſun-beams paſſed through red glaſs, have very little action upon it: yellow and green are more efficacious; but blue and violet light produce the moſt decided and powerful effects*.

Coloured rays act with different intenſities.

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* The facts above mentioned are analogous to thoſe obſerved long ago by Scheele, and confirmed by Senebier. Scheele found, that in the priſmatic ſpectrum, the effect produced by the red rays upon muriate of ſilver was very faint, and ſcarcely to be perceived; whilſt it was ſpeedily blackened by the violet rays. Senebier ſtates, that the time required to darken muriate of ſilver by the red rays, is 20 minutes, by the orange 12, by the yellow 5 minutes and 30 ſeconds, by the green 37 ſeconds, by the blue 29 ſeconds, and by the violet only 15 ſeconds.—*Senebier ſur la Lumière*, Vol. III. p. 199.

Some new experiments have been lately made in relation to this ſubject, in conſequence of the diſcoveries of Dr. Herſchel concerning the inviſible heat-making rays exiſting in the ſolar beams, by Meſſrs. Ritter and Bockmann in Germany, and Dr. Wollaſton in England.

It has been aſcertained by experiments upon the priſmatic ſpectrum, that no effects are produced upon the muriate of ſilver by the inviſible

Hence the light that paſſes through a painting on glaſs will cauſe various tinges on the paper, &c. and afford a copy ;

The conſideration of theſe facts enables us readily to underſtand the method by which the outlines and ſhades of paintings on glaſs may be copied, or profiles of figures procured, by the agency of light. When a white ſurface, covered with ſolution of nitrate of ſilver, is placed behind a painting on glaſs expoſed to the ſolar light ; the rays tranſmitted through the differently painted ſurfaces produce diſtinct tints of brown or black, ſenſibly differing in intenſity according to the ſhades of the picture, and where the light is unaltered, the colour of the nitrate becomes deepeſt.

or profiles may be taken from ſhadows.

When the ſhadow of any figure is thrown upon the prepared ſurface, the part concealed by it remains white, and the other parts ſpeedily become dark.

For copying paintings on glaſs, the ſolution ſhould be applied on leather ; and, in this caſe, it is more readily acted upon than when paper is uſed.

The tinge is permanent.

After the colour has been once fixed upon the leather or paper, it cannot be removed by the application of water, or water and ſoap, and it is in a high degree permanent.

The copies thus made are changeable by daylight ;

The copy of a painting, or the profile, immediately after being taken, muſt be kept in an obſcure place. It may indeed be examined in the ſhade, but, in this caſe, the expoſure ſhould be only for a few minutes ; by the light of candles or lamps, as commonly employed, it is not ſenſibly affected.

and this imperfection has not yet been remedied.

No attempts that have been made to prevent the uncoloured parts of the copy or profile from being acted upon by light, have as yet been ſucceſsful. They have been covered with a thin coating of fine varniſh, but this has not deſtroyed their ſuſceptibility of becoming coloured ; and even after repeated waſhings, ſufficient of the active part of the ſaline matter will ſtill adhere to the white parts of the leather or paper, to cauſe them to become dark when expoſed to the rays of the ſun.

Copies of textures thus made.

Beſides the applications of this method of copying that have been juſt mentioned, there are many others. And it

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inviſible heat-making rays which exiſt on the red ſide, and which are leaſt refrangible, though it is powerfully and diſtinctly affected in a ſpace beyond the violet rays out of the viſible boundary. *See Annalen der Phyſik, ſiebenter Band*, 527.—D.

will be uſeful for making delineations of all ſuch objects as are poſſeſſed of a texture partly opaque and partly tranſparent. The woody fibres of leaves, and the wings of inſects, may be pretty accurately repreſented by means of it, and in this caſe it is only neceſſary to cauſe the direct ſolar light to paſs through them, and to receive the ſhadows upon prepared leather.

Prints not very diſtinct.

When the ſolar rays are paſſed through a print and thrown upon prepared paper, the unſhaded parts are ſlowly copied; but the lights tranſmitted by the ſhaded parts, are ſeldom ſo definite as to form a diſtinct reſemblance of them by producing different intenſities of colour.

The images of the camera are too faint for this proceſs.

The images formed by means of a camera obſcura, have been found to be too faint to produce, in any moderate time, an effect upon the nitrate of ſilver. To copy theſe images was the firſt object of Mr. Wedgwood, in his reſearches on the ſubject, and for this purpoſe he firſt uſed the nitrate of ſilver, which was mentioned to him by a friend, as a ſubſtance very ſenſible to the influence of light; but all his numerous experiments as to their primary end proved unſucceſsful.

Thoſe from the ſolar microſcope are better.

In following theſe proceſſes I have found, that the images of ſmall objects, produced by means of the ſolar microſcope, may be copied without difficulty on prepared paper. This will probably be a uſeful application of the method; that it may be employed ſucceſsfully however, it is neceſſary that the paper be placed at but a ſmall diſtance from the lens.

To make the ſolution.

With regard to the preparation of the ſolution, I have found the beſt proportions thoſe of 1 part of nitrate to about 10 of water. In this caſe, the quantity of the ſalt applied to the leather or paper, will be ſufficient to enable it to become tinged, without affecting its compoſition, or injuring its texture.

The muriate is better than the nitrate as to delicacy,

In comparing the effects produced by light upon muriate of ſilver, with thoſe produced upon the nitrate, it ſeemed evident, that the muriate was the moſt ſuſceptible, and both were more readily acted upon when moiſt than when dry, a fact long ago known. Even in the twilight, the colour of moiſt muriate of ſilver ſpread upon paper, ſlowly changed from white to faint violet; though under ſimilar circumſtances, no immediate alteration was produced upon the nitrate.

The

but is leſs ſoluble.

The nitrate, however, from its ſolubility in water, poſſeſſes an advantage over the muriate: though leather or paper may, without much difficulty, be impregnated with this laſt ſubſtance, either by diffuſing it through water, and applying it in this form, or by immerſing paper moiſtened with the ſolution of the nitrate in very diluted muriatic acid.

Caution.

To thoſe perſons not acquainted with the properties of the ſalts containing oxide of ſilver, it may be uſeful to ſtate, that they produce a ſtain of ſome permanence, even when momentarily applied to the ſkin, and in employing them for moiſtening paper or leather, it is neceſſary to uſe a pencil of hair, or a bruſh.

Something like a dying proceſs may perhaps be effected here.

From the impoſſibility of removing, by waſhing, the colouring matter of the ſalts from the parts of the ſurface of the copy which have not been expoſed to light, it is probable that, both in the caſe of the nitrate and muriate of ſilver, a portion of the metallic oxide abandons its acid to enter into union with the animal or vegetable ſubſtance, ſo as to form with it an inſoluble compound. And, ſuppoſing that this happens, it is not improbable but that ſubſtances may be found capable of deſtroying this compound, either by ſimple or complicated affinities. Some experiments on this ſubject have been imagined, and an account of the reſults of them may poſſibly appear in a future number of the Journals. Nothing but a method of preventing the unſhaded parts of the delineation from being coloured by expoſure to the day is wanting, to render the proceſs as uſeful as it is elegant.

VII.

Obſervations and Experiments upon Oxigenized and Hyperoxigenized Muriatic Acid; and upon ſome Combinations of the Muriatic Acid in its Three States.* By RICHARD CHENEVIX, *Eſq. F. R. S. M. R. I. A. From the Philoſophical Tranſactions for* 1802.

Inference Berthollet, the ſo call igenated m of potaſh c tains more gen than i the uncom acid.

WHEN Mr. Berthollet made known the combination of what was then called oxigenated muriatic acid with potaſh, he gave as his opinion, that the proportion of oxigen, relatively to the quantity of acid, was greater in the ſalt than in uncombined oxigenized muriatic acid. This conjecture was fairly founded upon the obſervation, that, in his mode of preparing this ſalt, a large portion of common muriate was formed in the liquor, along with the hyperoxigenized muriate. The memoir which he publiſhed in the year 1788, is the laſt with which I am acquainted, upon this ſubject. It does not contain any thing that, conſidering the accuracy which is now required in experiments, amounts to a demonſtration of the relative proportions of oxigen, in oxigenized and hyperoxigenized muriatic acids. Unfortunately, this chemiſt has not purſued his reſearches any farther; although, from his own words, we had every reaſon to hope that they would have been continued.

Fourcroy's count.

In the *Syſtème des Connoiſſances Chimiques* of Mr. Fourcroy, we find a ſummary of the experiments that had preceded the impreſſion of his work, together with the following ſentence: "Tous les muriates ſuroxigénés ſont décompoſés par les "acides, ſouvent avec une violente décrépitation, avec un "dégagement de vapeur jaune verdâtre, et une odeur très-"forte. Cette vapeur eſt de véritable acide muriatique ſur-"oxigéné. Elle eſt lourde, tombe en goutellettes d'un jaune "vert, et forme des ſtries comme de l'huile, ſur les corps aux-"quels elle adhère." This aſſertion carries no confirmation along

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* I have preferred this word to *oxigenated*, becauſe *ate* is the appropriate termination of certain ſalts formed by acids in *ic*. Some further remarks upon this ſubject will be made in a work now in the preſs, intitled, *Remarks upon Chemical Nomenclature*.

exiſtence of
rox. mur.
not yet
ed.

along with it; and does not amount ſo near to proof as the poſition of the former chemiſt: ſo that, in fact, the exiſtence of hyperoxigenized muriatic acid, and of its combination with potaſh, reſts, at preſent upon the conjecture of Mr Berthollet; a conjecture however which, as well as his whole diſſertation upon the ſubject, bears all the marks of genius which ſo ſtrongly characteriſe every production of that ſagacious philoſopher. Some notice has been taken of other ſaline combinations, formed by cauſing a current of oxigenized muriatic acid to paſs through ſolutions of the alkalis, or earths, or by otherwiſe combining them. Meſſ. D'Olfus, Gadolin, Van-Mons, Lavoiſier, and others, have ſlightly mentioned ſome of theſe combinations. But, with the exception of Mr. Berthollet, I know of no chemiſt who has approached ſo near to the real ſtate of the combination of muriatic acid and oxigen with potaſh, as Mr. Hoyle, of Mancheſter. The true nature of this ſalt, however, is one of thoſe things which many perſons have credited without proof; and which many others have been on the eve of diſcovering.

v obs. and
ts. in proof
hat ſtate.

I ſhall now proceed to lay before the Society, an account of the obſervations and experiments which have led me to conclude, that muriatic acid does exiſt in the form of oxigenized and hyperoxigenized muriatic acid, as announced in the title of the preſent communication; and that, in either ſtate, it is capable of entering into ſaline combinations.

ler of narra-

With this view, I ſhall deſcribe,

1ſt, The means by which I think I have ſucceeded, in aſcertaining the conſtituent parts, as well as the proportions, in oxigenized and hyperoxigenized muriatic acid.

2dly, I ſhall mention ſome of the combinations of the muriatic acid, in its three ſtates.

In treating upon the firſt of theſe objects, I muſt in ſome meaſure anticipate the ſecond; and muſt ſuppoſe ſome things known, which are hereafter to be deſcribed. This inconvenience is inevitable; as the natural order of things leads me to treat of the acid, before I conſider the bodies into the compoſition of which it enters.

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ained in fu-
, and loſt

I expoſed to the heat of a lamp, 100 grains of hyperoxigenized muriate of potaſh. It decrepitated gently, and in a ſhort time melted. After remaining in fuſion nearly an hour, I allowed it to cool; it cryſtallized as formerly, and had loſt 2,5

2,5 per cent. I increaſed the heat to redneſs, in a furnace. The ſalt boiled with a violent effervefcence, and rapid diſengagement of gaſeous fluid, together with a thin white vapour, and then ſunk ſuddenly into a white ſpongy maſs. The loſs of weight uſually varied from 42 to 48 or 50 per cent.

0,035. Red heat cauſed eſcape of gas and loſs of 0,5.

Diſtillation of hyperox. mur. of potaſh, afforded oxigen; as and left muriate.

I put 100 grains into a coated glaſs retort, luted to a ſmall and perfectly dry receiver, having a tube communicating with a glaſs bell in the pneumatic tub. The fire had not been lighted very long, when a ſlight dew began to line the inſide of the receiver; and, as ſoon as the retort was nearly red-hot, a diſengagement of gas, ſo ſudden as almoſt to be exploſive, took place. A quantity of thin white vapour aroſe, which afterwards was depoſited, in the form of a white ſublimate, in the receiver and the tube. When the extrication of gas had ceaſed, the apparatus was allowed to cool. The gas, with the uſual corrections of temperature and preſſure, meaſured 112,5 cubic inches, = 38,3 grains. The 2,5 mentioned above, as the loſs of this ſalt at a low heat, were water. 53,5 remained in the retort; and the white ſublimate in the tube and receiver amounted to 5.

The products of this operation were therefore,

Water - - - - -	2,5
Oxigen - - - - -	38,3
Salt in the tube and receiver - -	5
Salt remaining in the retort -	53,5
	99,3.

To find the proportions of oxigen and muriatic acid, in hyperoxigenized muriatic acid, it now only remains to determine the ſum of the quantities of muriatic acid, contained in the 53,5 of the retort, and the 5 of the tube and receiver. The 53,5 gave, by nitrate of ſilver, a precipitate correſponding to 18,21; and the 5, a precipitate correſponding to 1,76; in all, 20 of the muriatic acid. Therefore, 38,3 of oxigen, and 20 of muriatic acid, combine to form 58,3 of hyperoxigenized muriatic acid; or, 100 of hyperoxigenized muriatic acid contain, within a fraction,

The reſidual muriate was employed to precipitate nitrate of ſilver, and its quantity of mur. acid inferred from the precipitate.

Oxigen - - - -	65
Muriatic acid - - -	35
	100.

The hyperox. muriate therefore contained nearly 1 part common acid, 2 parts potaſh, and 2 parts oxigen.

And the elements of hyperoxigenized muriate of potaſh, ſhould be thus ſtated:

Oxigen -	38,3	hyperoxigenized muriatic acid	-	58,3
Muriatic acid	20			
		Potaſh -	-	39,2
		Water -	-	2,5
				100,0.

Muriates loſe a little acid by ignition.

It may be obſerved, that the 53,5 of the retort did not yield the ſame proportion of acid as the 5 of the tube and receiver. The fact is, that all muriates loſe a little of their acid at a red heat, as I ſhall preſently mention more particularly; and the ſmall loſs was, in all probability, owing to a portion of acid diſengaged by the heat to which the ſalt was neceſſarily expoſed during the operation.

To determine the oxigen added to convert muriatic into oxyg mur. acid.

Having thus aſcertained the proportion of oxigen in hyperoxigenized muriatic acid, by means of its combination with potaſh, a ready method occurred to arrive at the knowledge of that contained in oxigenized muriatic acid. For this purpoſe, I diſpoſed in the following manner, a Woulfe's apparatus, conſiſting of three bottles, and connected with the pneumatic tub. In the firſt bottle, I put a ſolution of potaſh *, in about ſix parts of water. In the ſecond, a ſolution of the ſame; but ſo dilute, as that no part of the ſalt, which might be formed, ſhould cryſtallize during the operation. About twenty parts of water was the proportion there employed. In the third bottle, I put common carbonate of potaſh. Through this apparatus, I ſent a current of oxigenized muriatic acid, diſengaged by ſulphuric acid, from a mixture of muriate of ſoda and black oxide of mangaueſe, in the well known manner. Cryſtals of hyperoxigenized muriate of potaſh were formed in the liquor of the firſt bottle; and, as long as they remained, I was certain, from previous experiment, that no ſulphuric or muriatic acid could paſs into the ſecond bottle. The current was continued, till the liquor of that bottle contained an exceſs of acid. The carbonate of potaſh, in the third bottle, abſorbed the ſuperabundant vapours; and the pneumatic apparatus was ready to collect any gaſes that might be evolved. By theſe means, I

Ox. mur. acid gas was paſſed in ſucceſſion through ſtrong ſolution of potaſh, weak ſolution of potaſh, and ſolution of carbonate of potaſh.

The firſt bottle exhibited cryſtals and detained all the ſulph. or mur. acid.

The ſecond bottle received only hyperox. mur. acid, and contained its diſſolved combination with potaſh.

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* Whenever potaſh, ſoda, barytes, an acid, an alkali, water, or the names of other ſubſtances are uſed without an epithet, they are meant to denote them in that ſtate which is commonly called *pure*.

obtained, in the ſecond bottle, a ſolution of whatever ſubſtance might reſult from the action of potaſh upon hyperoxigenized muriatic acid.

The third bottle abſorbed the ſuperabundant vapours.

I took a portion of this liquor, which I ſhall call *entire liquor* [*], and diſtilled it to dryneſs in a glaſs retort, taking care to ſcreen it from light. A tube from the receiver communicated with the pneumatic tub. My object was to aſcertain, whether the change obſerved by Mr. Berthollet, in the diſtribution of the elements of oxigenized muriatic acid, to form, with potaſh, a ſimple and a hyperoxigenized muriate, really took place among thoſe elements themſelves, independently of any abſorption of oxigen from the atmoſphere, or extrication of it from the ſalt. Nothing but ſome water, and a few inches of the dilated air of the veſſels, paſſed into the receiver and the pneumatic apparatus; and I found, in the retort, a ſaline maſs [†], perfectly dry and cryſtallized. Hence it is evident, that the ſame quantity of oxigen as that formerly contained in the oxigenized muriatic acid, which had been united to the alkali, to form the total maſs of ſalt, was now condenſed, in that part which had become hyperoxigenized muriate.

The liquor of the ſecond bottle gave no gas by diſtillation, but left the dry cryſtalliſed ſalt.

To aſcertain this quality, I diſſolved 100 grains of the entire ſalt in water, and precipitated by nitrate of ſilver. I thus obtained a quantity of muriate of ſilver, which, by proportions previouſly determined, I knew to correſpond to 84 of muriate of potaſh: therefore, 16 were hyperoxigenized muriate of potaſh [‡]. But, according to the proportions eſtabliſhed above in hyperoxigenized muriate of potaſh, 16 of this ſalt contain 6 of oxigen, with 3,20 of acid, the remainder being alkali and water; and, by preliminary experiments, I found that 84 of muriate of potaſh contained 27,88 of muriatic acid. Therefore $27,88+3,20=31,08$ of muriatic acid, with 6 of oxigen, or, to reduce it to the quintal,

100 grains of this ſalt precipitated as much ſilver as 84 grains of muriate of potaſh would have done.

Therefore 16 parts were hyperoxigenized muriate.

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[*] I am well aware that, upon philoſophical principles, this appellation is objectionable; but, for the ſake of brevity, I have uſed it as a temporary name for a ſubſtance which has only a relative exiſtence among chemical bodies.

[†] This ſalt I ſhall call *entire ſalt*.

[‡] I muſt obſerve here, that hyperoxigenized muriate of potaſh does not, like ſimple muriate, decompoſe the ſalts of ſilver. This ſhall be further animadverted upon, and proved, in its proper place.

Muriatic acid - -	84
Oxigen - - -	16

100, are the proportions which combine to form oxigenized muriatic acid.

Confirmation by diſtilling off the oxigen.

To corroborate this evidence, I diſtilled 100 grains of the intire ſalt mentioned above; and obtained nearly 16,5 cubic inches of oxigen gas; which as accurately correſponds with the trials by nitrate of ſilver, as can be expected in experiments of this nature.

Compariſon with the experiments of Berthollet;

Mr. Berthollet, in his memoir upon oxigenized muriatic acid, gives, if I underſtand him rightly, the following ſtatement of the proportions, and of the means by which he obtained his reſults. He expoſed to the light of the ſun, 50 cubic inches of water, ſaturated with oxigenized muriatic acid; and collected in the pneumatic tub, 15 cubic inches of oxigen gas. I here neglect fractions; becauſe our reſults appear, at firſt ſight, to differ ſo widely as not to require great accuracy in giving their comparative ſtatement. He then precipitated, by nitrate of ſilver, the 50 cubic inches of liquor, which had become ſimple muriatic acid, and obtained 383 grains of muriate of ſilver. But, by experiments, I found that 383 grains of muriate of ſilver contain 65 of muriatic acid. Therefore, 65 of muriatic acid combine with 15 cubic inches [*] (= 8 grains) of oxigen, to form 73 of oxigenized muriatic acid. But 73 : 8 : : 100 : 11, or nearly. For this difference, however, it may be eaſy to account. Perhaps Mr. Berthollet's 50 cubic inches of oxigenized muriatic acid, contained originally a little ſimple muriatic acid; and he ſays beſides, that he ſuſpects all the oxigen was not diſengaged. This indeed is moſt probable; and I am happy that I can reconcile the proportions which I have found, to the opinion of ſo ſkilful a chemiſt.

and Cruickſhank.

Mr. Cruickſhank likewiſe, in his additional Obſervations upon Hydrocarbonates, has ſtated that 2,3 parts of oxigenized muriatic acid contain 1 of oxigen, or about 43,5 per cent. But this able chemiſt, to whom we are indebted for the diſcovery of the gaſeous oxide of carbon, procured his oxigenized muriatic acid by a peculiar method, which I ſhall notice in ſpeaking of the action of acids upon hyperoxigenized muriate of potaſh. The ſubſtance he obtained was, in fact, not oxigenized muriatic acid gas, but a mixture of that gas with hyperoxigenized muriatic acid. I have not the ſmalleſt doubt of the accuracy of his ſtatement; but, being the proportion of a mixture, it in no way contradicts either of thoſe I have determined in this Paper.

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[*] Mr. Berthollet's proportions are in the old French weights and meaſures.

There was no efferveſcence or loſs of oxigen in the experiments.

It is probable the efferveſcence of Berthollet was owing to carbonic acid.

Before I diſmiſs this part of the ſubject, I wiſh to anticipate an objection, founded upon an obſervation of Mr. Berthollet, which may be made to the above experiments. He ſays, that when the alkaline ſolution is very concentrate, an efferveſcence takes place during the whole of the ſaturation; and for ſome days after; and this efferveſcence he attributes to the eſcape of oxigen. But I have already ſaid, that no oxigen gas was diſengaged in any part of my proceſs; and no efferveſcence took place in any of the bottles, except the third; ſo that no ſuperabundance of oxigen could have paſſed from one into the other, nor could any diminution of the total quantity have been produced. By repeating the experiments, ſometimes with a ſolution of alkali, and ſometimes with water alone, in the firſt bottle, I obtained the liquor in the ſecond bottle uniform in all caſes. Indeed, as potaſh prepared in Mr. Berthollet's manner, was not in ſuch general uſe at the time he performed his experiments as at preſent, I ſuſpect that a great part of this efferveſcence was owing to a diſengagement of carbonic acid from the alkali.

Having thus proved the difference between the ſtates of theſe two acids, I ſhall now proceed to the combination of each with ſalifiable baſes.

OXIGENIZED MURIATES.

Whether the liquor before evaporation did contain the two ſalts or only one oxigenized ſalt, before the inſtant of cryſtallization.

As many properties of the entire liquor, before it had been evaporated to dryness, had led me to imagine that the acid was united with the alkali, and remained in combination with it, in the ſtate of oxigenized muriatic acid, till the moment of cryſtallization, I think it neceſſary to ſtate at length the appearances which induced me to draw that concluſion, and the experiments which afterwards convinced me that it was erroneous.

Sulphuric or acetic acid diſengaged oxyg. m. acid from the liquor;

A few drops of ſulphuric acid, poured into ſome of the intire liquor, cauſed an efferveſcence; and a ſmell of oxigenized muriatic acid.

but not from the ſalt.

Very ſtrong acetic acid produced the ſame effect.

By other experiments, I had aſcertained that acetic acid could not decompoſe any part of the intire ſalt; and hence I concluded, that in the intire liquor, before evaporation, ſome of the ſalt remained in the ſtate of oxigenized muriate, the acid of which was expelled by the ſulphuric or acetic acid; and, that it was not till the moment of cryſtallization that the elements of the ſalt underwent a total reſolution into muriate, and hyperoxigenized muriate of potaſh. However, a ſmall quantity of any of the very ſoluble neutral ſalts, ſuch as nitrate or muriate of ammonia, or even a little alcohol, produced the ſame effects; and I was then convinced, that the effervescence was owing to ſome uncombined oxigenized muriatic acid gas remaining in the liquor; and which was diſengaged, in proportion as the water was taken from it, by the ſuperior affinity of the ſalt, or the alcohol, I had uſed.

This however ariſes from ſome diſengaged exceſs of oxyg. mur. acid; for neutral ſalts could extricate it.

Acetic and acetous acids do not decompoſe hyperox. m. of potaſh;

but ox. m. acid decompoſes acetite of potaſh.

By ſome previous experiments I had aſcertained, as I have juſt mentioned, that acetic or acetous acids do not decompoſe hyperoxigenized muriate of potaſh. I ſent a current of oxigenized muriatic acid through a ſolution of acetite of potaſh; and, upon evaporation, I found that the acetous acid had been diſengaged, and that muriate, with hyperoxigenized muriate, of potaſh had been formed. But, from ſome trials, which I ſhall preſently relate, I was induced to believe, that oxigenized muriatic acid attracts the ſalifiable baſes with a much weaker affinity than acetous acid. It is well known that the contact of oxigenized muriatic acid with an alkali, is ſufficient to produce a combination of that acid with the alkali; and, from the laſt-mentioned experiments it appears, that it is not abſolutely neceſſary that the alkali ſhould be in a free ſtate. If it be combined with an acid weaker than hyperoxigenized muriatic acid, the original acid will be expelled; and muriate and hyperoxigenized muriate will be formed, as if the alkali had been free.

In ſuch diſengagements the alkali becomes combined as if it had at firſt been free.

As a further proof, that the change in the diſtribution of oxigenized muriate of potaſh takes place at the moment of contact of the acid and the alkali, and conſequently long before the cryſtallization, I mention the following experiments.

I precipitated, by nitrate of ſilver, 400 grains of the entire liquor, previouſly to its being evaporated; and obtained 71 grains of muriate of ſilver.

The firſt liquor was precipitated by nitrate of ſilver;

I evaporated to dryneſs, 400 grains of the ſame liquor, rediſſolved the reſiduum, and, by dropping in nitrate of ſilver, obtained 70 grains of muriate. The difference of one grain, in theſe experiments, does not amount to 0,2 of a grain of muriate of ſilver; and ought not to be regarded.

and afforded the ſame reſult as a ſolution of the intire ſalt.

From theſe experiments it is paſt all doubt, that the original intire liquor did not contain oxigenized muriate of potaſh. For, if ſuch a combination had exiſted in it, I ſhould have obtained a ſmaller portion of muriate of ſilver in the firſt than in the ſecond caſe, on account of the total ſeparation into muriate and hyperoxigenized muriate having not yet taken place.

Hence the firſt liquor did not contain oxig. m. of potaſh; but the two diſtinct ſalts.

We are not however to conclude, from theſe experiments, that there are no ſuch things as oxigenized muriates. Although they cannot be exhibited in a palpable ſtate, it is eaſy to demonſtrate that they do really exiſt. I ſhall prove, in the proper place, that hyperoxigenized muriate of ammonia is not an incompatible combination; and muſt, for the preſent, aſſume the datum, in order that I may demonſtrate the neceſſary exiſtence of oxigenized muriates. Therefore: If muriatic acid, or if hyperoxigenized muriatic acid, be brought in contact with ammonia, the reſult will be muriate, or hyperoxigenized muriate, of ammonia. But, if the acid, diſengaged by ſulphuric acid, from a mixture of black oxide of mangaueſe and muriate of ſoda, be ſent through ammonia, both are decompoſed. Hence it is evident, that the acid combines with the alkalis, in the ſtate of oxigenized muriatic acid; and that the ſeparation into muriate and hyperoxigenized muriate, is produced by a ſubſequent action, among the elements of oxigenized muriate of potaſh.

Yet it is poſſible to form oxig. muriates, for example, of ammonia.

For the oxigenized acid can decompoſe ammonia, and the hyperox. m. acid does not; and this laſt does, in fact, form a ſalt with it in the experiment.

Upon the whole, it appears to me fair to conclude,

1ſt, That the ſalts of this genus do really exiſt, previous to the formation of hyperoxigenized muriate of potaſh.

2d, That the affinity exerciſed by hyperoxigenized muriatic acid for ammonia, and (by very ſtrong analogy) for the other baſes, is much greater than that of oxigenized muriatic acid. For, hyperoxigenized muriatic acid, as ſhall preſently be ſhewn, having a much more powerful action upon all combuſtible

Conſequently there muſt have been an oxig. muriate firſt, and this was immediately, by ſtronger affinity, ſucceeded by the other ſalt, inſtead of decompoſition enſuing.

bodies, whether ſimple or compound, than oxigenized muriatic acid, it would be natural to ſuppoſe that the former acid would act more powerfully upon the inflammable element of ammonia. But oxigenized muriatic acid combines with the hidrogen of that alkali; which, however, is not decompoſed by hyperoxigenized muriatic acid; yet the affinity of hyperoxigenized muriatic acid for ammonia, is the only cauſe that determines the union of the acid and the alkali, without decompoſition. But theſe affinities ſhall be more fully treated of, in ſpeaking of hyperoxigenized muriate of ammonia.

ALKALINE AND EARTHY HYPEROXIGENIZED MURIATES.

Generic Characters.

Alk. and earthy hyperox. muriates. Generic characters.

Hyperoxigenized muriates are formed by paſſing a current of oxigenized muriatic acid through the baſis, diſſolved or ſuſpended in water, as in the formation of the laſt-mentioned genus. The firſt formation is owing to the ſeparation of the elements of an oxigenized muriate, into hyperoxigenized muriate and ſimple muriate; from which latter, they may be ſeparated by chryſtallization, or by another proceſs, which I ſhall mention, in treating of the earthy hyperoxigenized muriates. By ſimple trituration, they ſcintillate, with noiſe. They are decompoſed by a low red heat; and give out a conſiderable quantity of oxigen, as they become ſimple muriates. They cannot be brought down, by any means that I have tried, to that diminiſhed ſtate of oxigenizement, which would conſtitute oxigenized muriates. They inflame all combuſtible bodies with violence, as is well known. They are ſoluble in water; many of them, in alcohol; and ſome are deliqueſcent. The acid is expelled, with particular phenomena, by ſulphuric, nitric, and muriatic acids, without heat; and, a little below a boiling heat, by phoſphoric, oxalic, tartareous, citric, and arſenic acids: but they are not acted upon by benzoic, acetic, acetous, boracic, pruſſic, or carbonic acids. Thoſe vegetable acids which are powerful enough to decompoſe them, give out, towards the end, a gas of a peculiar nature, which has not ſo much ſmell as oxigenized muriatic acid gas, but which affects the eyes in an extraordinary manner, and promotes an uncommon and rather painful ſecretion of tears. I have not yet examined this gas, as there was invariably an inflammation of the mixture, with exploſion and rupture of the veſſels, almoſt as ſoon as it began to be evolved. When pure, the hyperoxigenized muriates do not precipitate any of the metallic ſalts, although I believe they decompoſe ſome. The order in which the baſes ſeem to be attracted by the acid, is, potaſh, ſoda, barytes, ſtrontia, lime, ammonia, magneſia, alumina, ſilica. The other earths I have not tried, and but few of the metallic oxides.

Firſt Species. Hyperoxigenized Muriate of Potaſh.

Hyperox. mur. of potaſh.

This ſalt is the beſt known of all the ſaline combinations of this acid. It has been erroneouſly conſidered as ſimply oxigenized, for its acid is really hyperoxigenized. It is ſoluble in about ſixteen parts of cold water, but in much leſs of warm; and is eaſily ſeparated, by cryſtallization, from muriate of potaſh. Alcohol can diſſolve a ſmall portion of it. It ſeems capable of exiſting in more ſtates than one; for, in paſſing a current of oxigenized muriatic acid, very ſlowly, and in the dark, through a ſolution of potaſh, till ſaturated, I have obtained flexible and ſhining needle-like cryſtals. This leads me to ſuſpect, either a hyperoxigenized muriate of potaſh with exceſs of acid, or that acid with a ſuperaddition of oxigen. It would be ſuperfluous to enter into a minute deſcription of a ſubſtance ſo well known as hyperoxigenized muriate of potaſh; but, it being the ſubſtance whence I have chiefly attempted to diſengage the acid, I ſhall enter into a particular detail of the action of the more powerful acids upon this ſalt.

Probably capable of exiſting in various ſtates.

Attempt to decompoſe it by ſulph. acid added to the ſalt.

Peculiar vapour.

If concentrate ſulphuric acid be poured upon hyperoxigenized muriate of potaſh, a violent decrepitation, ſometimes but rarely accompanied by a flaſh, takes place. A thick heavy vapour, of a greeniſh yellow colour, which riſes with difficulty to the top of the veſſel, if it be deep, is diſengaged. The ſmell is not altogether unlike nitrous gas; but it is peculiarly fetid, and may be compared to that which is emitted by brick-kilns, mixed with that of nitrous gas. It differs much from oxigenized muriatic acid gas; the latter being pungent and penetrating, the other heavy and oppreſſive; and it does not produce, at leaſt in ſo great a degree, the catarrhal ſymptoms cauſed by the other. At the bottom of this vapour is a bright orange-coloured liquor, which has the ſame ſmell as the vapour. This is the acid contained in the ſalt; and I have proved it to be hyperoxigenized muriatic acid. But, although the ſalt from which the acid is diſengaged be pure, the acid itſelf is never ſo; becauſe the very act of diſengaging it effects its decompoſition, and ſome of it is converted into oxigenized muriatic acid. The colour of litmus paper, on this account, is generally deſtroyed by the liquor. I ſay on this account, becauſe I have ſome reaſon to believe, from having obſerved this not to be uniformly the caſe, that hyperoxigenized muriatic acid reddens the vegetable blues. However, it muſt be conſidered, that the ſulphuric acid uſed to diſengage the hyperoxigenized muriatic acid is ſtill preſent; and we can draw no certain concluſion, until we have obtained this acid free from all other ſubſtances. If to this mixture of hyperoxigenized muriate of potaſh and ſulphuric acid, heat be applied, an exceedingly violent exploſion, with a white and vivid flaſh, takes place, before the liquor has attained the temperature of 125 of Fahrenheit. In order to obtain this acid, I attempted to diſtil 500 grains, in a glaſs retort, in a water bath, with every precaution againſt ſuch accidents as I could not but in ſome meaſure expect; when, almoſt as ſoon as I had kindled the fire, I ſaw, in the bottom of the retort, an extremely white, vivid, and rapid flaſh, which was immediately followed by a loud report. The retort was reduced almoſt to powder; ſo that ſcarcely any fragments of it could be found in the laboratory. The windows, and ſeveral glaſs veſſels, were broken. I happened to be holding the neck of the retort, at the moment of the exploſion, yet received no injury, except a ſlight contuſion in the hand. But Dr. Vandier, a French gentleman of conſiderable chemical and medical talents, to whom I am indebted for much able aſſiſtance in my laboratory, was wounded in ſeveral places; particularly, the tunica conjunctiva of the eye was ſo lacerated, that a piece of it hung down, and, by getting under the inferior eyelid, cauſed the moſt painful irritation, and endangered his ſight. One of the frontal arteries alſo was divided. I relate theſe circumſtances thus fully, as the moſt effectual means of putting upon their guard thoſe who would repeat the experiment. If the ſulphuric acid be dilute, heat may be applied with more ſafety; and the phenomena are different. The hyperoxigenized muriatic acid is diſengaged

The hyperox. m. acid thus diſengaged is not pure.

Heat produces violent and dangerous exploſion.

Narrative of an accident of this nature.

Dilute acid applied.

gaged from the baſis; but, as the heat requiſite to diſtil the acid is more than ſufficient to decompoſe it, oxigenized muriatic acid comes over with it; and oxigen gas is collected in the pneumatic tub. If the diſtillation be continued, the ſame danger ariſes as in the former caſe, becauſe the ſulphuric acid becomes concentrated; and it would ſeem, that its action upon the ſalt is ſlight and partial at a low temperature, but violent and inſtantaneous when heated and concentrate. I could not, therefore hope, by theſe means, to obtain the acid diſengaged and pure.

The ſalt added to the acid.

If the manner of bringing the ſulphuric acid and the ſalt into contact be reverſed, and the ſalt be dropped into the acid, the yellow vapours and the orange-coloured liquor are produced, but generally without decrepitation. If they be allowed to remain ſome days in contact, the vapours continue, and oxigen gas is conſtantly diſengaged, even in the common light of the day, and at the temperature of the atmoſphere. By cooling the firſt receiver with ice, I thought that I had once obtained this acid cryſtallized in the form of four-ſided pyramids, of an orange colour. But, though I really believe this to have been the caſe, I do not poſitively affirm it.

Nitric acid applied.

Nitric acid produces nearly the ſame phenomena; but the ſmell and other properties are rather leſs diſtinct and marked, than with ſulphuric acid.

Muriatic acid.

Muriatic acid decompoſes this ſalt, and unites to its baſis; but neither the yellow vapours, nor the orange-coloured liquor, are produced. The circumſtances which attend the contact of the acid and the ſalt, are as follows. If no more muriatic acid be preſent than is merely neceſſary to decompoſe the ſalt, I do not doubt that hyperoxigenized muriatic acid will be driven off, as little decompoſed as with the other acids, ſuppoſing the action to be inſtantaneous; but, during the contact of theſe two bodies, the acid expelled muſt meet muriatic acid not yet combined, and, uniting with it, always forms a portion of oxigenized muriatic acid. The quantity of the laſt acid muſt vary, according to the quantity of muriatic acid employed, and not combined with the alkali. It was by this method that Mr. Cruickſhank obtained the muriatic acid gas, which he ſtated to contain 43,5 per cent. of oxigen.

Phoſph. and acetic acids diſengage oxigen by heat.

Phoſphoric and arſenic acids do not act upon this ſalt, till heated with it; and then much oxigen gas is evolved. Theſe, therefore, afford no better method of diſengaging hyperoxigenized muriatic acid without decompoſition.

Oxalic, tartareous, and citric acids.

Oxalic, tartareous, and citric acids, act as I before mentioned; and the hyperoxigenized muriatic acid holds its place, in the order of affinities for potaſh, immediately before the benzoic.

Amuſing combuſtions by this ſalt and ſtrong acids.

I ſhall not ſtop to detail a number of amuſing phenomena that may be produced, by projecting into the ſtronger acids, mixtures of combuſtible bodies, whether metallic or not, and hyperoxigenized muriate of potaſh. The cauſe of them is well underſtood, and the theory points them out: they are, therefore, no longer objects of philoſophical admiration. But I muſt mention one experiment, which, had it ſucceeded, I ſhould have thought important. I projected various mixtures of very minutely pulverized diamond and this ſalt, into the different acids; but found the diamond undiminiſhed, by every attempt to combine it with oxigen in the humid way *.

The diamond does not burn in this humid way.

The h. ox. muriates contain much caloric.

Another, but imponderable, part of this ſalt, as indeed of all hyperoxigenized muriates, ſeems to be an extraordinary quantity of caloric. For, during their formation, ſcarcely any heat is diſengaged, as by other acids; and, very little heat applied to the ſalts, gives the gaſeous form to their oxigen.

Erroneous opinion, that nitrous gas may be formed from this ſalt, &c.

An opinion has prevailed among ſome ingenious chemiſts, that, from a mixture of this ſalt with ſulphuric acid, nitrous gas is diſengaged, and ſulphate of lime formed in the retort. But this is a miſtake, ariſing, on the one hand, from the ſmell and vapour of the hyperoxigenized muriatic acid, and, on the other, from ſulphate of lead, which the common ſulphuric acid of this country frequently contains in ſolution, and which is precipitated from it by water. Before we aſſert a fact, we ſhould be well aſſured of the pureneſs of our chemical agents. This

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* I muſt confeſs, that the vivid flaſhes of light, emitted from the mixture of this ſalt and combuſtible bodies thrown into an acid, appear to me, in ſome meaſure, to prove the modification propoſed by Leonhardi, Richter, Gren, &c. to that part of the Lavoiſierian theory which regards the emiſſion of light during combuſtion. Another teſtimony in favour of their modification, may be drawn from the vegetable kingdom. All plants growing in places deprived of light, are merely mucilaginous. But the mucilage of theſe plants burns without the emiſſion of light. Light, therefore, appears not to be diſengaged from oxigen; elſe, why not by this mucilage as well as by other combuſtible bodies?

ſuppoſed converſion of muriatic or hyperoxigenized muriatic acid into nitrous gas, will not paſs for a decompoſition, or a tranſmutation, of that refractory radical; and the idea of the change of potaſh into lime, is as erroneous as ſome other late aſſertions reſpecting the decompoſition of the alkalis.

The proportions of this ſalt are, as I before ſtated,

Component parts of hyp. ox. mur. of potaſh.

Hyperoxigenized muriatic acid -	58,3
Potaſh - - - - -	39,2
Water - - - - -	2,5
	100,0.

Second Species. Hyperoxigenized Muriate of Soda.

Hyper ox. mur. of ſoda. Very difficult to be ſeparated, becauſe of the little difference of ſolubility.

This ſalt is prepared in the ſame manner, and with the ſame phenomena, as the former. It is extremely difficult to obtain it pure, as it has nearly the ſame degree of ſolubility in water as muriate of ſoda. It is ſoluble in three parts of cold, and leſs of warm water; and is ſlightly deliqueſcent. It is ſoluble in alcohol; but this property alone is not ſufficient to enable us to obtain it free from the muriate of ſoda, formed along with it in the entire liquor; as the latter ſalt, contrary to the aſſertions of all authors, is ſoluble in alcohol, and ſeems to be much more ſo, when accompanied by the hyperoxigenized muriate. It was by taking a large quantity of the entire ſalt, formed by ſending a current of oxigenized muriatic acid gas through a ſolution of carbonate of ſoda, and repeatedly cryſtallizing in alcohol, that, with great difficulty, I obtained a little pure hyperoxigenized muriate of ſoda. It cryſtallizes in cubes, or in rhomboids little different from cubes. It produces a ſenſation of cold in the mouth; and its taſte is eaſily diſtinguiſhed from muriate of ſoda. It is decompoſed by heat, by combuſtible bodies, and by acids, in the ſame manner as the former ſpecies; and the acid holds its place for ſoda, as for potaſh, immediately before the benzoic. The baſis is ſeparated by potaſh only. This ſalt is compoſed of,

Obtained by cryſtallizing in alcohol.

Component parts.

Hyperoxigenized muriatic acid -	66,2
Soda - - - - -	29,6
Water - - - - -	4,2
	100,0.

(The Remainder in our next.)

VIII. *Deſcription*

VIII.

Deſcription of the Indian Hand Mill. From a Correſpondent.

General account of the hand mill.

THE Indian hand mill, like moſt other Indian inventions, is characteriſed by conſiderable ſimplicity and effect. Its cheapneſs and its general utility are ſuch, that the meaneſt hut in India is never without one. The whole of the grain uſed by the natives of India is ground in theſe mills, and chiefly by the women, who appear to execute the taſk with aſtoniſhing eaſe. A woman will continue grinding with this mill for ſeveral hours, and in this time ſhe will reduce a very conſiderable quantity of grain into flour.

Deſcription.

The mill is compoſed of two circular ſtones, an upper and an under one, each about two feet in diameter, and $2\frac{1}{2}$ inches in thickneſs. The quality of the ſtone nearly reſembles that of our common mill ſtones.

Figure and fitting of the under ſtone.

The upper face of the under ſtone is dreſſed perfectly flat. A ſmall hole, of nearly an inch in diameter, is ſunk in the center of this ſtone: this hole is fitted with a plug of hard wood, firmly driven into it, which contains a ſmall ſteel gudgeon or pivot. The ſurface of this wooden plug is ſlightly elevated above the face of the ſtone, and the gudgeon projects about half an inch beyond it. This gudgeon fits into a hole in the middle of a croſs bar, or flat piece of iron, eight or nine inches in length, about $\frac{3}{4}$ of an inch in breadth, and $\frac{1}{4}$ of an inch in thickneſs: the ends of this croſs bar are let into notches in the upper ſtone, and it thus forms a center on which the upper ſtone may revolve.

The upper ſtone.

The lower face of the upper ſtone is dreſſed perfectly flat, to correſpond with the upper face of the under ſtone. This ſtone is $2\frac{1}{2}$ inches in thickneſs at the edges, but it has a projection of nine inches diameter in the middle, at which place it is five inches in thickneſs. The ſtone is pierced in the middle by a hole of ſix inches in diameter, which ſerves as a hopper to contain the grain. In the upper face of this ſtone a hole is ſunk near the circumference, in which hole a wooden handle, about ſix inches in length, is firmly driven. By means of this handle the ſtone may be made to revolve on its gudgeon with conſiderable facility.

The projection of the wooden plug in the under ſtone in which the gudgeon is inſerted is ſo regulated as to keep the ſtones at the proper diſtance for forming the fineſt flour: if a coarſer ſort is wanted, it is readily obtained by placing one or more ſlips of paper or card in the notches of the upper ſtone, between the croſs bar and the ſtone. How the fineneſs of the flour is regulated.

The under ſtone is uſually placed on the earthen floor of the hut, which is previouſly beaten and ſwept, or otherwiſe it is placed on a cloth ſpread on the floor. As the grain is reduced to flour it eſcapes, together with the bran, at the circumference of the ſtones, and is collected on the floor or on the cloth. Local ſituation or poſition.

I would ſuggeſt as an improvement of this ſimple machine, the forming the croſs bar with three claws, which would give three points of bearing inſtead of two, and conſequently render the upper ſtone more ſteady *. I would alſo ſuggeſt, that the block of wood which contains the gudgeon, ſhould paſs completely through the under ſtone; thus it might eaſily be adapted to the requiſite height by a few blows of the mallet. Should it be thought neceſſary, the ſtones might be encloſed in a drum, and fitted on a ſtand. Improvements ſuggeſted.

Explanation of the ſketch, Plate X. Reference to the figures.

Fig. 1. Repreſents the lower face of the upper ſtone.

Fig. 2. Repreſents the upper face of the ſame.

Fig. 3. Is an elevation of the mill.

Fig. 4. Is a ſection of the mill.

a, Is the croſs bar, which is repreſented with three claws.

b, Is the handle by which the upper ſtone is turned.

c, Is the ſteel gudgeon on which the upper ſtone revolves by means of the croſs bar.

d, Is the ſmall block of hard wood in which the gudgeon is fixed.

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* It is likely that this ſteadineſs would do harm. The totter in all probability anſwers the ſame purpoſe, of pounding, as the vertical dancing motion of the upper mill ſtone upon the ſupport of its pivot, of which Deſaguliers has given a very rational account. Lectures II. 428.

IX. *An*

IX.

An Effect hitherto unpubliſhed, by which the two Electricities are diſtinguiſhed. In a Letter from Mr. JOHN CUTHBERTSON,

To Mr. NICHOLSON.

DEAR SIR,

Letter.

ON reading your note reſpecting the inſtruments by which the two kinds of electricity are diſtinguiſhed, or its direction aſcertained, it brought to my mind an experiment which I made with that view in the year 1777 in Holland; I believe it is not known here, ſo perhaps you may think it worthy of a place in your valuable Journal.

I am,
Dear SIR,
your's reſpectfully,
J. CUTHBERTSON.

Poland Street, Soho, Oct. 19, 1802.

Experiment. A candle between two balls in the oppoſite ſtates of electricity has its flame much agitated, and heats the negative ball only.

INSULATE two wires furniſhed at each end with a metal ball of about $\frac{1}{4}$ of an inch diameter, connect one with the poſitive conductor, and the other with the negative conductor of an electrical machine; ſet them ſo that their balls may be at about four inches diſtance, place between them a common ſized lighted candle, with the center of its flame nearly upon a level with the centers, and at an equal diſtance from each; if the machine be put in motion the flame will waver very much, and ſeems rather more to incline to the negative ball than to the poſitive one, but is very equivocal: continue turning (if the machine be a plate of two feet diameter) about 50 revolutions, then the negative ball will begin to grow warm, and the poſitive ball remain cold; if the revolutions be multiplied to 200, the negative ball will be too hot to be touched, and the poſitive remain as cold as at the beginning.

X.

Palpable Mathematics; or Methods of writing and calculating for the Uſe of the Blind. By J. B. Berard.*

Citizen Haüy having compared the methods of writing and calculating of ſeveral celebrated blind men, has digeſted into a body of doctrine the beſt productions of experience in this art. He has compoſed a ſyſtem of inſtruction, and has deſcribed the inſtruments proper to be uſed in this operation: and he truly deſerves well of humanity, who increaſes the means of communication between ſociety, and a claſs of men who ſeem to have been excluded from it, and to have been condemned to lead an unhappy life, uſeleſs to others, and without enjoyment to themſelves.

Citizen Haüy's inſtructions to the blind as to writing, &c.

Having been myſelf deprived of ſight ſince the age of twenty-three, meditation and experience have given me the unhappy right of treating upon the art concerning which he has written. I here give ſome obſervations which may not be entirely uſeleſs to ſociety.

The author is blind.

ARITHMETIC.

The method by which the celebrated Saunderſon calculated is well known; however ingenious it may be, it does not ſeem to me to be the beſt; I obſerve two principal defects in it.

Profeſſor Saunderſon's method of computing

He uſed ſmall ſquare pieces with nine holes in each: the poſition of a pin in one of theſe holes denoted one of the nine figures. It is therefore neceſſary to have as many of theſe ſquares as the operation requires of figures or characters: and we muſt employ an extended ſpace for a very ſimple calculation.

by ſquares to denote the digits,

In the ſecond place, this method of calculating being entirely different from that of perſons who can ſee, they cannot underſtand the particulars of it; this is a conſiderable inconvenience, becauſe it deſtroys all connection between the blind and thoſe who operate by ſight.

required a great ſpace, and is not legible to others.

The method which appears to me the beſt, conſiſts in employing characters repreſenting figures, and arranging them nearly as they are compoſed in printing.

It is better to compoſe as printers do,

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* Melanges Phyſico Mathematiques.

ALGEBRA.

and in algebra likewiſe.

The beſt method of writing algebra appears to me, like that of arithmetic, to conſiſt in compoſing in the manner of printers, with this difference, that the characters are a little larger, and muſt be read from left to right. This proceſs has the important advantage of enabling readers by ſight to peruſe and examine the calculations.

GEOGRAPHY.

Eaſy method of forming tangible maps;

There is but one method of uſing geographical maps and the globes: it is ſimple and eaſy. A ſmall ſtring or thread is paſſed on all the windings or outlines of the map. The rivers and mountains are diſtinguiſhed by different threads. Grains or prominences of different ſizes and ſhapes are paſted on the towns and other remarkable places, &c. With one of theſe maps others may be procured by the preſs, in which the outlines will be raiſed into relief.

and multiplying copies.

MUSIC AND BOOKS.

Muſic books.

Citizen Haüy has compoſed muſic books for the uſe of the eſtabliſhment of the *Quinze Vingts* at Paris, the characters of which project, or are emboſſed: this method is without doubt uſeful, but it is ſcarcely applicable but to great foundations.

WRITING.

Writing on paper with a blunt ſtile is legible by the protuberance.

Citizen Haüy's method conſiſts in uſing an iron pen, the point of which is not ſplit; by writing without ink and preſſing on a ſtrong paper, the blind man produces a character in relief, which he can immediately read by paſſing his fingers over the projecting characters on the oppoſite ſide of the paper, in the contrary direction. This relief is ſufficient, provided a ſoft ſurface be placed under the paper, ſuch as leather, blotting paper, &c.

Frame for writing ſtraight.

In order to write in a ſtraight line, a frame is put on the paper, having a number of braſs wires acroſs, parallel to the direction of the writing, at the diſtance of about three quarters of an inch.

His method of writing in relief has this advantage, that the blind can read what they have written, after a little practice;

but it has the inconvenience of preventing perſons that can ſee from reading it, unleſs they have alſo practiſed the ſame method *.

How to make the writing *viſible.*

A black writing may likewiſe be obtained with this iron pen, by placing on the writing paper another ſheet done over with hogs-lard and ivory black, covered again by another, through which the letters are traced. The inconvenience of this method is, that the blind cannot read what they have written.

Inſtead of the frame with parallel threads, I employ a board which has ſeveral conveniences.

A more convenient frame for ſtrait lines.

This board is about 10 inches in breadth, and 16 high; on the left ſide, and near half an inch from the edge, there is a ſmall dove-tailed groove from top to bottom. A rule three quarters of an inch wide, and 4½ long, ſlides in this groove by a piece ſo formed as to fit the groove. Another rule five eighths of an inch wide, is fixed perpendicularly to the firſt rule in the form of a T, and ſerves very conveniently to direct the little finger of the hand that writes. Laſtly, the left ſide of the board is divided into notches almoſt half an inch diſtant from each other: this being done, the following is the method of writing:

Method of writing with ink.

The paper is placed under the long rule, and is faſtened by two ſcrews eaſy to be imagined, which preſs it againſt the board: the writing is performed by keeping the hand lightly over the rule, the little finger being behind. The rule is ſhifted from line to line very readily by the left hand, the thumb being applied to feel the notches. At the finiſh of every line ink is dipped or taken (in a ſilver pen formed in the uſual method) out of a broad flat veſſel conſtantly containing ink to the depth of between one eighth and one quarter of an inch.

By this proceſs, the lines may be written as near each other as may be required. The rule directs the hand better than a wire, which is too flexible, and not broad enough. When, after diſcontinuing writing the writer is deſirous of proceeding, the rule indicates the point to begin from, &c.

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* If the paper be rubbed with pumice, or have ſome earthy powder in the ſize, and a blunt point of hard pewter be uſed, the letters will be neat and viſible.---N.

INK IN RELIEF.

Deſideratum; a viſible and tangible ink.

It would be a very uſeful invention, if an ink could be formed which in coming from the pen ſhould leave a mark that would project. This ink would have the double advantage of enabling the blind and thoſe who ſee to read at the ſame time writing, the figures of arithmetic, the characters of algebra, and the notes of muſic. I have not yet thought of any ſatisfactory expedient for this purpoſe, and ſhall not therefore enter into any further detail.

GEOMETRY.

Saunderſon's method of forming geometrical figures with pins and threads. A better method; by wires on a flat cuſhion.

Saunderſon, in order to conſtruct geometrial figures, placed pins in holes made in a board, and paſſed threads from one pin to another. My method appears to be preferable in every reſpect; it is this: with four ſides of wood I have conſtructed a frame, the length of which is about 20 inches, the breadth 16, and thickneſs three quarters of an inch. I have covered the two faces with woollen cloth rather open and ſoft, and have filled the intermediate ſpace with other looſe and ſoft woollen cloth, ſo as to form a flat cuſhion three quarters of an inch thick.

I have very ſmall iron wires, bended at right angles at the two ends, which are pointed and poliſhed. Theſe iron wires are eaſily fixed in the cuſhion, and form lines in relief, which nothing can derange. I have an aſſortment of all lengths, as well as circles, ſemi-circles, parabolas, ellipſes, &c. Laſtly, I have letters in relief, with a point in order to fix them on the cuſhion.

This method is very ready and comprehenſive.

By means of this little apparatus, there is no figure in geometry, however complicated, but may very ſpeedily be repreſented in relief.

We might likewiſe, and with advantage, uſe this cuſhion for making figures with pins and threads; but this method, though better than that of Saunderſon, is not ſo good as the iron wires here deſcribed. Experience is deciſive in this reſpect.

and is applicable to muſic, &c.

It is eaſy to perceive that the above method is likewiſe the beſt for the notes of muſic; but the limits I have adopted will not allow me to enter into further details.

XI. *Obſervations*

XI.

Obſervations on the real Nature of the Precipitates formed by the Pruſſiates in Acid Solutions of Barites, and the Affinities of the Pruſſic Acid [*]. *By* CITIZEN GUYTON.

Precipitation of barites by Pruſſian alkali conſidered as an indication of its metallic nature.

BERGMAN announced, that the acid ſolutions of barites are precipitated by the ſaturated Pruſſian alkali. This fact, confirmed by the obſervations of a great number of chemiſts, appeared to many as a new indication of its metallic nature, already ſuſpected on account of its great weight. Lavoiſier in particular thought it very probable, that the difficulty of reducing it to the metallic ſtate proceeded from its having more affinity with oxigen than carbon [†].

This notion abandoned,

and the precipitation by pure pruſſiate denied.

The little ſucceſs of the attempts to effect this reduction having in ſome meaſure cauſed the notion of this compoſition to be abandoned, the attention was again fixed by the phenomenon of the precipitation of an earth by a re-agent, which according to the generally received opinion, afforded a characteriſtic indication of the preſence of a metal. It was then ſuſpected to be owing to ſome accidental cauſe. M. Meyer of Stetin, announced in 1786, in Crell's Annals, that the pruſſiate of potaſh, if very pure and carefully prepared, did not precipitate barites from its ſolutions. Kirwan and Klaproth adopted this opinion from their own experiments. Moſt chemiſts however continued to believe that the precipitation took place, whatever might be the means of purification of the proof liquor. Pelletier, amongſt others, ſuppoſed it one of the moſt eſſential diſtinctive characters of barites and ſtrontian [‡].

Carbonate of potaſh precipitates pruſſiate of lime,

Some time ago being deſirous to try a pruſſiate of lime recently prepared with lime of marble, and very clear, I poured into it a ſolution of carbonate of potaſh. The liquor inſtantly became milky and opake. As it is certain that no metallic ſubſtance was preſent, I could only attribute this decompoſiby double affinity.

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[*] Read at the ſitting of the claſs of Philoſophical and Mathematical Sciences of the Inſtitute, the 2d Thermidor in the year 10, and inſerted in the Annales de Chimie, XLIII. 185.

[†] Traité Elementaire, &c. Vol. I. p. 174.

[‡] Mem. de Chimie, Vol. II. p. 454.

tion to a change of baſe of the two ſalts by double affinity. I then purpoſed to examine whether this might not be abſolutely the ſame phenomenon which had given riſe to the ſuſpicion of the metallic nature of barites.

Mr. Henry's experiments from the Philoſ. Journal.

I was anticipated in theſe reſearches by Mr. W. Henry. He has ſucceeded in placing this point of theory in the cleareſt light: I ſhall repeat in a few words the experiments that led him to it, and which he publiſhed in Mr. Nicholſon's Journal*; after which I ſhall make ſome reflections upon one of the conſequences he deduced from them, which appears to me to require amendment.

Pruſſiate of barites formed,

Mr. Henry's firſt object was the direct compoſition of pruſſiate of barites, in order to obtain a perfectly pure pruſſiate of potaſh: the following is his proceſs, which he affirms that he carried into effect, and was ſuggeſted to him by obſerving the decompoſition of the ſulphate and carbonate of potaſh by the pruſſiate of barites.

by calcining the carbonate of barites; diſſolving the earth in water, and add. Pruſſian blue till it is no longer diſcoloured by the earth ſeizing the p. acid.

He calcines the carbonate of barites; he diſſolves this earth in boiling water, and adds Pruſſian blue till it is no longer diſcoloured. The filtered liquor ſometimes becomes turbid by cooling, and depoſes a little oxide of iron. He filters it again, and at the end of ſeveral hours ſmall yellowiſh cryſtals are formed, which are the pruſſiate of barites. He obtains an additional quantity by evaporation.

The cryſtallized pruſſiate of barites is decompoſed by carbonate of potaſh, and thus gives fine cryſtals of pruſſiate of potaſh.

This pruſſiate of barites in powder is thrown into a hot ſolution of carbonate of potaſh, until it no longer reſtores the colour of blue paper. The author obſerves, that it is better to employ more pruſſiate than is neceſſary for the decompoſition of the carbonate. He digeſts the mixture for about an hour; then filters the liquor, evaporates it ſlowly, and obtains by this method very fine cryſtals of pruſſiate of potaſh.

Laſt purification by acetous acid.

Theſe cryſtals nevertheleſs ſometimes contain as much as 0.24 of oxide of iron; the greateſt part of this is ſeparated by digeſting the liquor before filtrated, with a little acetic acid, which has the advantage of forming with the potaſh an uncryſtallizable ſalt, and conſequently cannot mix with the cryſtals of the pruſſiate.

The precipitated carbonate of barites may again be uſed in the ſame operation.

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* Vol. IV. Quarto, p. 30, 170.

Let us now obſerve the uſe Mr. Henry made of the pruſſiate of potaſh thus prepared to reſolve the controverted queſtion.

He poured ſome of it into a ſolution of muriate of barites. No precipitation followed; the mixture was not in the leaſt diſturbed, and he was inclined to think with Meyer and Klaproth that a pure pruſſiate does not precipitate barites.

The pure pruſſiate of potaſh did not precipitate the muriate of barites at firſt;

But having obſerved the mixture half an hour afterwards, he perceived ſmall cryſtals forming on the ſides of the veſſel; and at the end of ſome hours they were conſiderably augmented.

but it did in half an hour.

Theſe cryſtals when examined preſented the following characters:

1. They are very ſparingly ſoluble in cold water; one ounce is required to diſſolve a quarter of a grain *.

Character of the precipitated cryſtals.

2. Warm water diſſolves them rather quicker, though in a very ſmall proportion.

3. Theſe ſolutions afford with ſulphuric acid and ſulphate of potaſh a precipitate of ſulphate of barites; and Pruſſian blue with ſulphate of iron.

They were undoubtedly pruſſiate of barites.

4. Theſe cryſtals are completely diſſolved in muriatic acid diluted with water, and this ſolution afforded unequivocal ſigns of the preſence of pruſſiate of barites.

5. Theſe cryſtals heated to redneſs in a ſilver crucible became black, and loſt their form; muriatic acid poured on the coaly reſidue produced an effervеſcence, and muriate of barites was formed.

Mr. Henry draws ſeveral conſequences from theſe facts. The *firſt*, that he was deceived, as well as thoſe who announced, that pure barites does not decompoſe pruſſiate of potaſh.

Inductions from theſe facts.

The *ſecond*, that barites ought to be placed before potaſh in the column of affinities of pruſſic acid.

The *third*, that the precipitation effected by pruſſiate of potaſh in a ſolution of muriate of barites, is the product of a double affinity.

The *fourth*, that *barites differs in this reſpect from the other earths, and approaches the metals.*

Of theſe four propoſitions, the three firſt ſeem to me to be open to no objection; for though Mr. Henry does not relate

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[*] As the Engliſh ounce is here ſpoken of, which contains 480 grains, cold water only diſſolves about 0,0005 of its weight.---G.

any direct experiment on this ſubject, it is known, and I have myſelf obſerved it, that the aqueous ſolution of barites poured into a ſolution of alcaline pruſſiate, decompoſes it by ſimple affinity.

Objection to the inference that barites approaches to the metals;

It is not the ſame with the laſt concluſion of Mr. Henry; for though theſe acid ſolutions of barites are decompoſed by the alcaline pruſſiates, preciſely like the metallic ſolutions by the way of double affinity, or the concurrence of divellent forces which determine the exchange of baſes, it in no reſpect follows that barites is on that account removed from the earths, or approached to the metals. If that were the caſe, the ſame muſt be aſſerted of all ſubſtances, which preſent the ſame phenomenon, and it would no doubt be a matter of ſurprize to find this reaſoning lead us to aſſimilate to the metals *lime, ſtrontian, magneſia, potaſh, ſoda,* and even *ammonia,* ſince the Pruſſic acid can take all theſe baſes from their ſolvents when the ſum of the divellent affinities is in its favour.

becauſe the precipitation is by double affinity, and happens with other earths and alkalies,

namely potaſh;

I have proved this in the caſe of *potaſh* by the experiment already related, where it is ſeen that it takes the Pruſſic acid from the lime, and yields to it the carbonic acid.

ſoda;

This effect is ſtill ſpeedier with carbonate of *ſoda,* and pruſſiate of lime.

ſtrontian;

The ſolution of nitrate of *ſtrontian,* is likewiſe decompoſed by the pruſſiate of lime.

lime;

If a few drops of pruſſiate of lime be poured into a ſolution of ſulphate of ammonia in five or ſix minutes, a whitiſh frothy film of the thickneſs of about a quarter of an inch thick ſwims at the ſurface, and is at laſt depoſed in ſmall flakes.

magneſia;

Laſtly, ſulphate of magneſia mixed with pruſſiate of lime becomes clouded, and a precipitate is formed.

and becauſe the order of affinity is not the only diſtinctive character of bodies.

Let us therefore return to the true cauſe of theſe phenomena, the action of the double affinities in which we ſtill trace the ſimple elective attractions, the order of which is totally independent of that ſeries, or arrangement in which we place ſuch ſubſtances as have ſeveral other properties in common. Therefore the ſtation which barites holds before potaſh in the columns of the attractions of pruſſic acid, ought no more to be applied in its claſſification, than the poſition of lime before the fixed alcalis in the columns of the oxalic, tartarous, and boracic acids, &c. With much more foundation it has been propoſed to rank barites with the alcalis, whoſe chemical action it in fact performs in many circumſtances, but from which it however differs, as I have elſewhere obſerved *, in ſome characters, which may be conſidered as limits, and particularly by its inſolubility in alcohol.

XII.

A Review of ſome Experiments, which have been ſuppoſed to diſprove the Materiality of Heat. By WILLIAM HENRY. *From the Memoirs of the Literary and Philoſophical Society of Mancheſter, Vol. V. Part II.*

Preface.

THE following remarks, on the ſubject of heat, were written ſoon after the publication of Count Rumford's Inquiry concerning the Source of the Heat evolved by Friction; and of the intereſting eſſays of Mr. Davy, which appeared in Dr. Beddoes's Weſt Country Contributions. They were tranſmitted to Dr. Beddoes, for publication, about the cloſe of the year 1799; but circumſtances, with which I am unacquainted, have, I believe, induced the Doctor to decline the continuation of his periodical work. Theſe circumſtances I deem it neceſſary to ſtate; becauſe, had the eſſay been written nearer the period of its publication, it would probably have aſſumed a very different form. At preſent, I have not leiſure to review the ſubject, or to attempt any material alteration; and ſtill leſs, to examine whether I have been anticipated by any of the authors whoſe eſſays have been publiſhed during the two laſt years.

A Review of ſome Experiments, which have been ſuppoſed to diſprove the Materiality of Heat.

Queſtion whether heat be matter or motion.

It has long been a queſtion among philoſophers, whether the ſenſation of heat, and the claſs of phenomena ariſing from the ſame cauſe, be produced by a peculiar kind of matter, or by motion of the particles of bodies in general. The former of theſe opinions, though far from being univerſally admitted, is now moſt generally received; and the peculiar body, to which the phenomena of heat are referred, has been demonſtrated by M. Lavoiſier, caloric. Againſt the doctrine of the French ſchool ſome forcible arguments have lately been advanced by Count Rumford and by Mr. Davy, both of whom have adopted that theory reſpecting heat, which aſſigns, as its cauſe, a motion among the particles of bodies.

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* Annales de Chimie, Vol. XXXI. p. 267.

Mr. Davy's oppoſition to the matter of heat is by *reductio ad abſurdum.*

The method of reaſoning, employed by Mr. Davy, in proving the immateriality of the cauſe of heat, is the *reductio ad abſurdum*, i. e. the oppugned theory is aſſumed as true, together with its applications; and facts are adduced, directly contradictory of the aſſumed principles. I ſhall take the liberty of offering a ſtatement of the argument, rather different from that of Mr. Davy; though I truſt without miſrepreſentation, or any material omiſſion.

Another ſtatement: Aſſuming the exiſtence of caloric, &c.

Let heat be conſidered as matter; and let it be granted, that the temperature of bodies depends on the preſence of uncombined caloric. Now, if the temperature of a body be increaſed, the free caloric, occaſioning that elevation, muſt proceed from one of two ſources; either, *1ſtly*, It may be communicated by ſurrounding ſubſtances; or, *2dly*, It may proceed from an internal ſource, *i. e.* from a diſengagement of what before exiſted in the body, latent or combined. But the temperature of bodies is uniformly increaſed by friction and percuſſion, and, neceſſarily, in one of the foregoing modes.

Heat by friction in vacuo,

I. Mr. Davy found, by experiment, that a thin metallic plate was heated by friction in the exhauſted receiver of an air-pump, even when the apparatus was inſulated from bodies capable of ſupplying caloric, by being placed on ice. This experiment he conſiders as demonſtrating, that the evolved caloric could not be communicated by ſurrounding bodies.

does not exclude caloric,

becauſe a vacuum conducts.

To the inference deduced from this experiment it may be objected, that the mode of inſulation was by no means perfect. Admitting the vacuum produced by the air-pump to have been complete, ſtill the ſupply of caloric could not thus be intirely cut off; ſince it has been ſhewn by Count Rumford, that caloric paſſes even through a torricellian vacuum. If, therefore, friction produce in bodies ſome change, which enables them to attract caloric from ſurrounding ſubſtances, this attraction may be equally efficient in an exhauſted receiver, as in one containing an atmoſphere of mean denſity. It would be an intereſting ſubject of experiment, to determine the influence of atmoſpheres of various denſities, as conductors of caloric; for, ſince effects are proportionate to their cauſes, and it is aſcertained that common air conducts caloric better than it is conveyed through a vacuum, as 1000 is to 702, it may be expected that the ratio will hold in all intermediate degrees.

In Count Rumford's experiments (Philoſ. Jour. 4to, II. 106), caloric might be ſuppoſed to be abſorbed and emitted at the ſame time.

In Count Rumford's maſterly experiment, the metal, ſubmitted to friction, was encompaſſed by water; and air was carefully excluded from the ſurfaces in motion. Yet the water became hot, and was kept boiling a conſiderable time. In this caſe, the only obvious ſource of caloric, from without, was through the borer employed in producing the friction; if it be true, as the Count has obſerved, that the water could not, at the ſame inſtant, be in the act of giving out and receiving heat. The ſame objection to the communication of heat, from an external ſource, exiſts alſo in thus explaining Mr. Davy's experiment: but I cannot admit that the argument is demonſtrative, in proving the evolved caloric not to be derived from external ſubſtances; for no abſurdity is implied in ſuppoſing, that a body may be receiving caloric in one ſtate, and giving it out in another. We have an example of the ſimultaneous admiſſion and extrication of a ſubtle fluid, the materiality of which is admitted by Mr. Davy, in an excited electric, which, at the very ſame inſtant, receives the electric fluid from without, and transfers it to the neighbouring conductors. In an ignited body, alſo, the two proceſſes of abſorption and irradiation of light, are, perhaps, taking place at the ſame moment.

Inſtance in electricity.

In bodies which emit heat by friction; but do not have their capacity diminiſhed;

II. Another cauſe of the increaſe of temperature in bodies, is the liberation of their combined caloric; and, if this be a ſource of temperature, the abſolute quantity of caloric in a body muſt be diminiſhed by friction. That no ſuch diminution really takes place, we have the evidence of two experiments—the one of Mr. Davy, the other of Count Rumford. Mr. Davy, by rubbing together two pieces of ice, converted them into water. Now water, *ex hypotheſi*, contains more caloric than the ice from which it was formed; and, on the ſame hypotheſis, the abſolute quantity of caloric in ice is diminiſhed by friction and liquefaction, which is abſurd. Count Rumford alſo aſcertained, that the ſpecific heat of iron was not diminiſhed when converted by a borer into turnings, and conſequently when it had been the ſource of much temperature. In explanation of theſe facts, we may be allowed to aſſume the communication of caloric from ſurrounding bodies, till this communication has been demonſtrated to be impoſſible. But even were the

it may be ſuppoſed the heat came from without;

the impoſſibility eſtabliſhed, it would yet remain to be proved, that the evolved caloric does not proceed from an internal ſource; and this can only be done by an accurate compariſon of the quantity of caloric in bodies, before and after friction. Now, in inſtituting this compariſon, it is implied, that we poſſeſs means of determining the abſolute quantity of caloric in bodies, and that we can compare quantities of caloric with as much certainty as we can obtain from an appreciation by weight or by meaſure. Such perfection however does not, I apprehend, belong to the preſent ſtate of our knowledge reſpecting heat; for I have always been diſtruſtful of that part of the doctrine, which aſſigns the ratio of heat latent in bodies. The grounds of this diſtruſt I ſhall ſtate pretty fully; for, if it can be proved that we have no accurate conceptions of quantity, as appertaining to heat, all arguments againſt its materiality, derived from ſuppoſed determinations of its quantity, muſt be inconcluſive.

and the experiments for deciding capacities are queſtionable.

It is aſſerted that our only clear conceptions of quantity are from magnitude or weight;

The only clear conceptions which the mind has of quantity, are derived either from a compariſon of the magnitude, or of the gravity of bodies. In the inſtance of caloric, both theſe modes of menſuration fail us. We cannot eſtimate the bulk of a ſubſtance which eludes our graſp and our viſion; nor have we yet ſucceeded in comparing its gravity with that of the groſſer kinds of matter, which it ſurpaſſes in tenuity beyond all compariſon. Our notions of the quantity of caloric are derived, not from ſuch ſimple judgments, but from complicated proceſſes of reaſoning, in the ſteps of which, errors, fatal to the whole, may perhaps ſometime appear.

neither of which can be exhibited in caloric.

Caloric is peculiar in its increments being denoted by expanſions.

Whatever be the nature of caloric, whether it be a body *ſui generis*, or a quality of other bodies, its effects are peculiar and appropriate; and, like all other facts, bear a proportion to the energy of their cauſe. Expanſion, for example, it is proved by experiment, keeps pace with the actual increments of heat; and on this principle is founded the thermometer, the great agent in the acquirement of all our ideas reſpecting heat, both abſolute and relative. The competency of this inſtrument, however, to afford information of the quantity of caloric, is limited by the following circumſtances:

The thermometer indicates no abſolute quantities,

1ſt, The mercury of the thermometer indicates only the quantity of heat which it has itſelf acquired, and by no means that contained in ſurrounding bodies. *2dly*, The ſcale of expanſion is wholly arbitrary, commencing far from the abſolute privation of heat, and falling far ſhort of its maximum. 3*dly*, The caloric, latent in bodies, or chemically combined with them, has no effect on the thermometer. 4*thly*, The experiments of Dr. Crawford, though ſufficient to ſhew that the expanſion of the mercury of the thermometer bears a ratio to the actual increments of heat, in any temperature between the boiling and freezing points of water, by no means prove that this proportion holds univerſally.

and the laws developed by Crawford apply only to a ſmall part of this ſcale.

Equal weights of heterogeneous bodies, it is preſumed, contain unequal quantities of caloric; and the ratio of theſe quantities is approximated in the following manner.

Equal weights of the ſame body, at different temperatures, give, on admixture, the arithmetical mean: but equal weights of different bodies, at different temperatures, afford a temperature which varies conſiderably from the mean. Thus a pound of water at 100 degrees, and a pound at 200°, give the temperature of 150°; but a pound of water at 200°, and a pound of mercury at 100°, afford, not the mean, but a temperature conſiderably higher. Hence it follows that a pound of mercury has not the power of fixing and retaining ſo much caloric as a pound of water: and the fixation of more heat, by the water than by the mercury, is aſcribed to the ſuperior energy of a power inherent in both, and termed capacity for caloric.

Common method of finding the ratios of caloric by the common temperature + or — than the mean.

From an extenſive ſeries of experiments Dr. Crawford infers, that the capacities of bodies are permanent, ſo long as they retain their form. Thus, the capacity of water has to that of mercury the ratio of 28 to 1, at any temperature between 32° and 212°. The difference of capacities of bodies, it is inferred, therefore would continue the ſame, down to the abſolute privation of temperature. Imagine, then, two bodies at this point of privation: they may ſtill contain unequal quantities of combined caloric; for, when chemically combined, caloric does not produce temperature. On Dr. Crawford's hypotheſis, theſe comparative quantities of combined caloric, in the two bodies, may be learned by obſerving the ratio of temperature, produced, by the addition to each, of ſimilar quantities of heat. This ſuppoſition, however, is manifeſtly gratuitous; and the contrary might be maintained with equal or greater probability: for it may be ſuppoſed that, at this aſſumed negation of temperature, one body renders latent

Capacities permanent by Crawford's hypotheſis,

not applicable at remote and unknown temperatures, not probable.

latent more caloric than another, becauſe it actually contains leſs; as certain dry ſalts attract more water from the atmoſphere than others containing much water of cryſtallization. The commonly employed mode of aſcertaining the ſpecific caloric of bodies, is ſounded, therefore, on an aſſumption, which is deficient in the character of a datum, and which itſelf requires proof.

Theorem for finding the abſolute zero, fallacious.

The method.

If theſe objections be valid, they will apply alſo to ſhew the fallacy of the theorem, for finding the abſolute zero of bodies. By this term ſome philoſophers appear to underſtand the point of abſolute privation of caloric, both free and combined. I apprehend, however, that in ſtrict propriety it can only be uſed to ſignify the negation of *uncombined* caloric, or, as Dr. Crawford expreſſes himſelf, the point of abſolute cold. As applied, however, to water, it is evident that the whole quantity of heat is underſtood. In aſcertaining the zero, ſay theſe calculators, the capacity of ice to that of water is as 9 to 10. It is plain, therefore, that when water freezes, it muſt give out $\frac{1}{10}$th of its whole heat, and this 10th part is found to anſwer to 146° of Fahrenheit. Conſequently its whole heat is 10 times 146, or 1460°; and hence the natural zero is 1460 — 32 or 1428°. Now of this eſtimate it is a datum, that the capacities of ice and water have preciſely the above ratio. But if the general formula, for aſcertaining the ſpecific caloric of bodies, be founded on erroneous principles, it cannot ſerve as the groundwork of any ſolid concluſions.

Arguments including theſe doctrines, therefore prove nothing either way.

The materiality of caloric may, I apprehend, be maintained, without admitting that we have made any ſteps towards determining its quantity in bodies; and the arguments of Count Rumford and Mr. Davy are not demonſtrative, becauſe they aſſume, that this part of the doctrine of caloric cannot be relinquiſhed, without abandoning it *in toto*. I may be permitted, therefore, to ſtate my reaſons for believing caloric to be matter; which would have been unneceſſary, had the contrary been proved, with all the force of mathematical demonſtration.

That caloric is matter;

becauſe it expands bodies, and therefore occupies ſpace, and is impenetrable:

Avoiding all metaphyſical reaſoning on the nature of matter, and aſſuming the generally received definition, as ſufficiently characterizing it, I ſhall examine how far this general character of matter applies to the individual—caloric. Caloric occupies ſpace or is extended, becauſe it enlarges the dimenſions of other bodies; and, for the ſame reaſon, it is impenetrable, ſince if it could exiſt, at the ſame time, in the ſame place, with other bodies, their volume would never be enlarged by the addition of heat. Of form or figure, as only a mode of extenſion, it is unneceſſary to prove that caloric is poſſeſſed; and indeed there is perhaps only one general quality of matter that will not be allowed it, viz. attraction. That caloric is influenced by the attraction of gravitation, or by coheſive attraction, has never yet been proved. Yet the various experiments of Buffon, Whitehurſt, Fordyce, Pictet, &c. cannot be alledged as proofs, that it is actually devoid of this property; ſince they only decide, that the ſmall quantities, which can be artificially collected, are not to be ſet in the balance againſt the groſſer kinds of matter. One kind of attraction, *that* which has lately been termed chemical affinity, may, I think, after a full ſurvey of phenomena, be fairly predicated of caloric—and if its poſſeſſion of this quality be rendered probable, we ſhall thence derive a powerful argument in favour of its materiality.

It has not been proved deſtitu of weight.

That chemical affinity has a conſiderable ſhare in producing the phenomena of heat, appears probable from the following conſiderations:

It appears to have chemical attraction;

1. All the characters, diſtinguiſhing caloric when ſeparate, ceaſe to be apparent, when it has contributed to a change of form in other bodies; and the properties of the ſubſtances ſo changed are alſo materially altered. Now this is the only unequivocal mark of chemical union that we can apply in any inſtance; and chemical union implies the exiſtence and efficiency of chemical affinity.

becauſe its pro perties appear combination;

2. The relation of caloric to different ſubſtances, appears to obſerve that peculiar law, which, in other inſtances, is termed elective affinity. If a compound of two or more principles, a metallic oxide for inſtance, be expoſed in a high temperature, the caloric forms a perfect union with the one, but not with the other. In certain inſtances, caloric is evolved when two ſubſtances, attracting each other more powerfully than they attract caloric, produce, on admixture, an elevation of temperature. In other inſtances, caloric is abſorbed when it is attracted by the new compound more ſtrongly than by the ſeparate components. Such facts warrant the deduction, that caloric is ſubject to the laws of chemical affinity. But the preciſe order of its affinities remains to be decided by future experiments.

and it ſhews elective affini ſingle;

and double or compound;

3. Caloric ſeems alſo, on ſome occaſions, to bear a part in the operation of double elective affinities. In this way it produces decompoſitions, which, by ſingle affinity, it is incapable of effecting. Thus a moſt intenſe fire does not expel intirely the carbonic acid from alkalis. But when the affinity of an acid for an alkali concurs with that of carbonic acid for caloric, a decompoſition enſues.—Again, water may be ſubmitted to the higheſt temperature, without imparting a gaſeous form to the hidrogen which it contains; but the conſpiring affinity of a metal for oxigen occaſions the production of hidrogenous gas. On this principle, many chemical facts are reſolved into the law of *double affinity*, which are, at preſent, explained by that of ſingle elective attraction.

and by intermedium.

4. Caloric acts, ſometimes, as an intermedium in combining bodies, which, without its aid, are not ſuſceptible of combination. Thus carbon and oxigen do not evince any tendency to combination, at the ordinary temperature of the atmoſphere; but caloric brings them into union, and conſtitutes, itſelf, part of the reſulting compound. This, and a variety of other inſtances, have a ſtriking reſemblance to what is called *intermediate affinity*.

Capacity and affinity are not ſynonimous terms.

In the theory of Dr. Crawford, no influence is allowed to chemical affinity over the phenomena of heat; and indeed *that* philoſopher expreſſes a decided opinion, that elementary heat is not capable of uniting chemically with bodies. Hence it appears, that the difference between the terms affinity and capacity is not merely a verbal one; but that they are actually expreſſive of different powers or cauſes: and the queſtion, therefore, which of theſe terms ſhall be adopted, in the deſcription of facts, is one involving the determination of cauſes.

On the word capacity as applied by Crawford.

The term capacity for heat is employed, by Dr. Crawford and others, to denote, in the abſtract, that power, by which different kinds of matter acquire different quantities of caloric. But in the various applications, that are made of this theory, a more preciſe meaning is often affixed to it; and the term is applied, in much the ſame ſenſe, which it has in common language. When thus underſtood, a difference of capacity neceſſarily implies a difference in the extent of the ſpaces, between the minute particles of bodies; and that theſe differences occaſion the varieties, obſerved in the acquirement of heat by different bodies. On this theory, there is no active principle or power inherent in bodies, and more active in ſome than in others,—no tendency in the matter of heat to attach itſelf, in preference to any one ſubſtance. The aſſigned cauſe of the phenomena of heat is not, I apprehend, adequate to produce the effects aſcribed to it.

It uſually implies no power but mere locality.

By this theory the change of capacity may precede the tranſition of heat.

On the theory of capacities, a change of form is, in certain inſtances, antecedent to the abſorption of caloric. Thus, when ether is converted into gas, on removing the preſſure of the atmoſphere, according to this hypotheſis, the capacity of the ether is increaſed by its volatilization; and the change of form is prior to, and the cauſe of, the abſorption of caloric. The order of events, then, in the volatilization of ether, is firſt an alteration of form; next a change of capacity; and laſtly an abſorption of caloric. On this hypotheſis, ether may exiſt in the ſtate of gas, without containing a greater abſolute quantity of caloric, than in a liquid form. But ſuch an interpretation of phenomena is directly contradictory to an eſtabliſhed principle, admitted, even by thoſe who prefer the doctrine of capacities, viz. that all bodies, *during* their converſion from a fluid to a vaporous ſtate, abſorb caloric. It is at variance, alſo, with obſerved facts: for if a thermometer be immerſed in a portion of ether, confined under the receiver of an air pump, the temperature of the ether will be found to ſink gradually, during the exhauſtion of the air; and the evaporation becomes proportionally ſlower, till, at laſt, it is ſcarcely perceptible. We may, therefore, infer, that at a certain point of diminiſhed temperature, the volatilization of ether would entirely ceaſe, if the ſupply of caloric, from ſurrounding bodies, could be completely intercepted. But on the theory of capacities, the evaporation ſhould proceed as rapidly at the cloſe, as at the commencement, of the proceſs —or, in other words, evaporation ſhould be wholly independent of temperature, which every one knows is contrary to fact.

Whence it is moſt probable that the effects ariſe from affinity.

It may be conſidered therefore, as extremely probable, that the tendency of ether to aſſume a gaſeous form depends on its chemical affinity for caloric. But, (it may be aſked) how is this affinity counteracted by an increaſed preſſure, and augmented by a diminiſhed one?

A cir-

Mechanical preſſure prevents the formation of gas; but is not an argument againſt this doctrine.

A circumſtance, abſolutely eſſential to the formation of gaſes, is, that free ſpace ſhall be allowed for their expanſion. Mechanical preſſure acts as a counteracting force to this expanſion; and either prevents it completely, or partially, according to the degree of its application. But from this fact, no argument can be drawn againſt the exiſtence of chemical affinity, as an attribute of caloric. Two oppoſite forces in phyſics may be ſo balanced, that neither ſhall produce its appropriate effect. Thus a body, impelled in contrary directions, may remain at reſt, yet the operation of the oppoſing forces, in this caſe, cannot be denied. Even in chemiſtry, we have unequivocal examples, in which the action of the affinities is ſuppreſſed by more powerful cauſes. Thus bodies, that have a ſtrong chemical affinity, are kept perfectly diſtinct, even when placed in contact, by the affinity of aggregation. The only inference, then, that can fairly be adduced from the effects of preſſure, in preventing the formation of gaſes, is, that it is a power, ſometimes ſuperior, in energy, to that of chemical affinity.

Caloric having all the properties of matter, but gravity may be taken to be matter.

Since, therefore, caloric is characterized by all the properties, except gravity, that enter into the definition of matter, we may venture to conſider it as a diſtinct and peculiar body. Nor is its deficiency of gravity ſufficient to exclude it from the claſs of material ſubſtances. Such nicety of arrangement might, with equal propriety, lead us to deny the materiality of light, the gravity of which has never yet been proved: for, beſides the experiments of Mr. Michell, which failed in aſcertaining this property of light, we have ſeveral chemical facts tending to the ſame concluſion. Thus Mr. Cavendiſh, after firing a mixture of hidrogenous and oxigenous gaſes, in a cloſe veſſel, a proceſs during which much light is always emitted, found not the ſmalleſt diminution of weight.

Phenomena of heat differ from thoſe of motion.

To have completed this defence of the material nature of heat, it would have been proper to have pointed out the circumſtances, in which the phenomena of heat differ from the known and acknowledged phenomena of motion. At preſent, however, I have not leiſure to purſue the ſubject at much length; and, though ſeveral points of diſagreement would doubtleſs be found, I ſhall mention only one of the moſt marked and deciſive.

Motion is an attribute of matter, independently of which it cannot poſſibly ſubſiſt. If therefore, the phenomena of heat can be ſhewn to take place, where matter is not preſent, we ſhall derive, from the fact, a concluſive argument againſt that theory of heat, which aſſigns motion as its cauſe. Now, in the experiment of Count Rumford, before alluded to, heat paſſed through a torricellian vacuum, in which, it need hardly be obſerved, nothing could be preſent to tranſport or propagate motion. This experiment, in my opinion, decidedly proves, that heat can ſubſiſt independently of other matter, and conſequently of motion—in other words *that heat is a diſtinct and peculiar body.*

Heat can paſs through a vacuum; but motion ſurely cannot be tranſmitted where there is no matter. Hence the heat is matter.

XIII.

Enquiries concerning the Dilatation of the Gaſes and Vapors. Read to the National Inſtitute of France. By Cit. Gay Lussac[*].

Art. I. *The Object of this Memoir.*

NATURAL philoſophers have been long engaged in reſearches on the dilatation of the gaſes; but their labours preſent ſuch variations in their reſults, that ſo far from having fixed the general opinion, they have on the contrary left us great reaſon to wiſh for more accurate inveſtigations.

The reſearches on the gaſes are inaccurate.

The attention of philoſophers has not been equally directed to the dilatation of vapors. Though the prodigious effects of the vapor of water has long been known and applied with the happieſt effect, Ziegler and Bettancourt are the only perſons who within my knowledge have attempted to meaſure them. Their experiments however are not adequate to ſhew the true dilatation of the vapor; for as there was always water in their apparatus, every new degree of heat not only produced a dilatation of the vapor formed by the preceding degrees, but alſo increaſed the volume by the formation of new vapors; two cauſes which evidently tend to raiſe the mercury in their manometer[†].

The expanſions of vapor have been little examined.

The

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[*] From the Annales de Chimie, XLIII. 187.

[†] The apparatus of Bettancourt conſiſts in a boiler of copper cloſed by a cover of the ſame metal, through which paſs three tubes.

The thermometer does not indicate the real quantities of heat.

The thermometer, as it is at preſent conſtructed, cannot be applied to point out the exact proportions of heat, becauſe we are not yet acquainted with the relation between its degrees and the quantities of heat. It is indeed generally thought that equal diviſions of its ſcale repreſent equal tenſions of caloric; but this opinion is not founded on any well decided fact.

Numerous demands in philoſophy and the arts for the knowledge of the expanſion of gas and vapor.

We muſt admit therefore, that we are ſtill far from poſſeſſing poſitive knowledge reſpecting the dilatation of the gaſes and vapors, and the correſpondent march of the thermometer; nevertheleſs, we have daily occaſion in natural philoſophy and chemiſtry to reduce a given volume of gas from one temperature to another, to meaſure the heat diſengaged or abſorbed in the changes of conſtitution of bodies; or that diſengaged or abſorbed by the ſame body in paſſing from one temperature to another; and in the arts to calculate the effects of fire engines, or to eſtimate the expanſions of various bodies; in meteorology to determine the quantity of water held in ſolution by the air, a quantity which varies with its temperature and its denſity, according to a law not yet aſcertained. Laſtly, in the conſtruction of tables of aſtronomical refraction, and in the application of the barometer to the meaſurement of heights, it is indiſpenſible that we ſhould accurately know the temperature of the air, and the law of its dilatations.

The author's inducements to this work.

Though theſe conſiderations were of themſelves ſufficient to render the enquiry into an object of ſuch general utility deſirable, yet the difficulty of the reſearches it demands would have prevented my attempting it, if I had not been ſtrongly engaged by Cit. Berthollet, whoſe pupil I have the honor to be. I am indebted to him for the neceſſary means of executing this work, in which I have often been aſſiſted by his advice,

The firſt ſerves to introduce water, the ſecond admits a thermometer to indicate the temperature of the vapor, and to the third is adapted a recurved barometrical tube, to meaſure the elaſticity of the ſame vapor. A vacuum is made in the boiler by means of an air pump communicating through a tube provided with a ſtop cock.

The apparatus of Ziegler differs little from that of Bettancourt; but as Ziegler did not make a vacuum in his boiler, there is a great difference in their experiments and reſults. See the Hydraulic Architecture of Prony, Vol. II.---G. L.

advice, and that of Cit. Laplace. Theſe great authorities will add to the confidence to which my work may be entitled.

The experiments relate only to the co
parative expa
ſion of differe
gaſes and vapo
at a given tem
perature.

The experiments which I have undertaken on the law of the dilatation of gaſes and vapors, and the march of the thermometer not being yet completed, the only object of this memoir will be to examine the dilatation of gaſes and vapors at a determined elevation of temperature, and to ſhew that it is the ſame in all theſe fluids. But before I relate my experiments, I think it proper to give a ſhort account of what has been done on this ſubject; and as I ſhall make obſervations on each of the methods that have been employed, I ſhall prefix this amount to the examination of one of the principal cauſes of uncertainty to which this ſpecies of experiments is liable. Although it is very important, and ſeems to have remained unknown to the generality of philoſophers who have examined the dilatations of gaſes, its influence will be fully admitted when ſimply pointed out. What I ſhall ſay of atmoſpheric air will apply to the other gaſes.

Water in the apparatus is th
great cauſe of uncertainty.

Explanation of this effect.

This cauſe of uncertainty is owing to the preſence of water in the apparatus. If a few drops of this fluid be left in a receiver full of air, and its temperature then raiſed to that of boiling water; this water when converted into vapor, will occupy a volume about 1800 times greater than its primitive bulk, and will drive off by this means a very conſiderable portion of the air contained in the receiver. It neceſſarily happens then, that when theſe vapors are condenſed, and come to occupy a volume 1800 times leſs, a dilatation much too great will be attributed to the air remaining in the receiver; becauſe it will be ſuppoſed that it was this air which at the temperature of boiling water occupied all the capacity of the receiver. If the temperature be not raiſed to this degree, the ſame cauſe of uncertainty will not the leſs exiſt, and its intenſity will be relative to the height of temperature: for in this caſe the water will not entirely evaporate, but the air will diſſolve more of it in proportion as its temperature is raiſed, and it will conſequently receive a greater or leſs augmentation of volume, independent of that occaſioned by heat; ſo that as it paſſes to a lower temperature, the volume of air which occupied the whole receiver will diminiſh for two reaſons, 1ſt. by the loſs of its caloric; 2d. by that of the water i held in ſolution. Too much dilatation will be here alſo attributed to the air.

Other liquids, or even ſolids may affect the reſult.

In general, whenever liquids, or even ſolids, as for example, muriate of ammonia are incloſed in gaſes capable of diſſolving them, or of evaporating at the temperature to which they may be expoſed, errors in the determination of the gaſeous dilatations muſt neceſſarily reſult.

ART. II. *Short Account of what has been aſcertained concerning the Dilatation of Gaſes.*

Hiſtory of the ſcience of the expanſions of gaſes, &c. Amontons.

The dilatation of atmoſpheric air by heat was known before the time of Amontons; but it appears that this philoſopher was the firſt who wiſhed to know the extent for a given elevation of temperature. For this purpoſe he encloſed air in a ball ſoldered to the extremity of one of the branches of a reverſed ſyphon, and he plunged this apparatus into a bath of hot water *. The air dilated by the heat compreſſed the mercury, and elevated it into the other branch of the ſyphon; ſo that he judged of the ſpring of the air by the height of the mercury above the level of the ball.

From the various experiments made on unequal volumes of air he concludes: (*Mem. de L'Acad.* 1699, 1702.)

Amontons's reſults on common air

1ſt. "That the heat of boiling water has limits, beyond which it does not paſs.

2d. "That unequal volumes of air equally augment the force of their ſpring by equal degrees of heat, and on the contrary.

3d. "That the heat of boiling water does not augment the force of the ſpring of air beyond the power of ſuſtaining about ten inches in height of mercury."

mechanically compreſſed.

He then proves, that however compreſſed a volume of air may be, the heat of boiling water always augments the force of its ſpring about one third; that is to ſay, that a volume of air compreſſed, for example, by a column of mercury of 60 inches, will ſuſtain (including the weight of the atmoſphere) at the temperature of boiling water, a column of mercury of

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* The air encloſed in the ball, not being able to eſcape when mercury is poured in, is found a little leſs in volume than it naturally is; but if no other preſſure is deſired than that of the atmoſphere, it will be very eaſy to avoid this ſlight inconvenience.

Much force from ſmall increaſe of temperature in condenſed air.

about 80 inches. He concludes from this, "That the ſame degree of heat, however ſmall it may be, can always more and more augment the ſpring of air, if this air bears a weight more and more great."

Amontons had not a comparable thermometer.

If Amontons had begun to reckon from a degree of heat better determined than that which he called *temperate*, which was then hardly poſſible, there might have been deduced from his experiments a ſufficiently exact dilatation of atmoſpheric air; nevertheleſs, as he made them comparatively on volumes of air of very unequal denſity, it may be concluded that *however denſe a volume of air may be, the augmentation of ſpring it receives from the ſame degree of heat, is always relative to that it poſſeſſed before the experiment.*

Nuguet's experiments differed from thoſe of Amontons,

Nuguet, in endeavouring to verify the reſults of Amontons, found others very different. In one of his experiments, the volume of air dilated by the heat of boiling water, and the primitive volume were as 2 to 1, and in two others as 16 to 1. His apparatus conſiſted of a bottle, which he plunged reverſed into a water bath, and elevated its temperature to that of boiling water. It is eaſily ſeen that this apparatus is extremely defective, as the air was always in contact with the water; and beſides, Nuguet had ſuffered ſome water to remain in the bottle. It is no wonder then that he obtained ſuch diſproportioned and extravagant reſults. (*Mem. de L'Acad.* 1708. Lahire.)

becauſe his air was in contact with water.

Lahire's repetition of Amonton's experiments.

This great difference between the reſults of Amontons and Nuguet on the dilatation of atmoſpheric air, and the conſideration that they had ſubmitted it to experiments under uncommon circumſtances, engaged Lahire to purſue the ſame object. His apparatus was the ſame as that of Amontons, excepting that the ball contained a tube, which he cloſed after having introduced mercury. By this means, the mercury being on a level with the ball and ſyphon, the air on which the experiment was made, was not more compreſſed than the ſurrounding atmoſphere. With this apparatus Lahire found, that air dilated from the degree of temperate to that of boiling water, could not ſuſtain a column of mercury of one third the weight of the atmoſphere; he then found in another, the thermometer being lower, and the barometer higher than in the firſt experiment, that the air dilated by the heat of boiling water, could not ſuſtain a column of mercury as great as in the firſt. Theſe reſults are evidently contradictory; but Lahire not ſuſpecting any error, concluded that it muſt be neceſſarily allowed that the nature of air was yet undiſcovered.

Lahire objects to the experiments of Nuguet on account of water.

Lahire, in order to give a reaſon for the difference between his reſults and thoſe of Nuguet, a difference much too great not to be owing to ſome foreign cauſe, remarked, that Nuguet had left ſome water in his apparatus; and from hence he judged that it might be this water, as it changed to vapor, and expelled a portion of the air encloſed in the bottle, had preſented ſo great a dilatation. He was entirely confirmed in his opinion by the reſult of an experiment made in Nuguet's method, a little water being left in the bottle; for he found that the volume of air dilated from the degree of temperate to that of boiling water, and the primitive volume was as 35 and a half to 1. (*Mem. de L'Acad.* 1708.)

Stancari alſo obſerved the influence of water.

At the ſame time M. Stancari of Bologne proved, that water at an elevated temperature augmented the volume of air. To theſe two philoſophers then is owing the knowledge of the influence of water on the dilatation of atmoſpheric air; but although they placed it by their experiments in the cleareſt light, it has been ſince generally miſconceived. The great variations in the reſults of philoſophers on the dilatation of gaſes, may likewiſe be attributed to the little attention that has been paid to this influence.

The dilatations of air are of conſequence in barometrical admeaſurements.

It is known that elevations in the atmoſphere are given by the logarithms of the correſpondent heights of the barometric column. If the denſity of the air were always the ſame, it would be eaſy to determine the height of one place above any other known place by barometrical obſervation. Whence it becomes of importance to diſtinguiſh the cauſes that may influence the denſity of the air, in order to make the neceſſary corrections in the heights given by the barometer.

Deluc on the barometric meaſurement.

Deluc, who has thrown ſuch great light on this branch of philoſophy, diſcovered that heat was one of the cauſes. That he might well diſtinguiſh its effects, he firſt ſought the temperature at which the logarithms give the heights without correction, and he found by comparing ſeveral obſervations made in ſituations whoſe heights he had exactly determined, that this takes place at the temperature of 16° ¾ of the thermometer divided into 80 parts, which he calls the fixed temperature. To correct the effects of heat above and below this fixed

Temperature of which the logs. give the height.

fixed point, he compared the heights found by the logarithms with thoſe which he had meaſured, by attributing to heat the exceſſes and deficiences of the firſt for the ſecond; and he concludes "that near the fixed temperature, the correction for a degree of the thermometer is to the height of the place :: 1 : 21 5" (*Recher. ſur les modif. de l'at.* IV. part. ch. III.)

Correction for temperature.

Colonel Roy diſcovered a much greater dilatation in air. According to him near the 15° of the thermometer divided into 80 parts, the air dilates $\frac{1}{172}$ of its volume for each degree. He likewiſe found that humid air dilates much more than dry air; but Sauſſure obſerves, that Colonel Roy having introduced into his manometer either liquid water, or the vapor of water, confounded two things which ought to be ſeparated, namely, the converſion of water into an elaſtic fluid, and the dilatation of air united with ſuch vapor. (Phil. Tranſact. 1777, p. 704.)

Colonel Roy's allowance greater, but he produced more vapor the greater his heat.

Sauſſure fixes the dilatation of air near the 6th degree, at $\frac{1}{235}$ of its volume for each degree. His experiments were made in a great receiver, in which were encloſed a thermometer and barometer, to indicate the variations of the temperature of the air, and the correſpondent elaſticity it would acquire. To diſcover the influence of water on the dilatation of air he encloſed in his receiver air more or leſs dry, and avoided the production of new vapors. He was ſo far from finding this air more dilatable than very dry air, that he on the contrary thought very dry air was a little more dilatable than very moiſt; but always holding its water in perfect ſolution. (*Eſſai ſur l'Hygrometrie*, p. 108.)

De Sauſſure's reſult. He avoided the production of new vapor; and in this caſe found dry air rather more dilatable.

Till this period philoſophers had confined themſelves to the dilatation of atmoſpheric air, and the firſt who examined that of the other gaſes was the celebrated Prieſtley. The following is his proceſs:

The gaſes were not yet examined. Prieſtley's experiments.

After having filled a phial of gas over mercury, he adapts to it a bended tube, one of its branches being very inclined, and he leaves a little of the mercury in the neck of the phial, that the expanſion of the gas may make it paſs into the tube. This done, he places his apparatus in a box of wood, joins a thermometer with it, and takes it into apartments of different temperatures. The dilated air cauſes the mercury to paſs through a greater or leſs ſpace in the tube, and by this ſpace meaſured

The gas was received over mercury, and its expanſion noticed in a recurved tube.

meaſured in inches, Prieſtley diſcovered the dilatations of the different gaſes. As all theſe experiments were made with the ſame phial and tube, which he probably always inclined in the ſame angle, they gave a comparative reſult between the dilatations of the different gaſes; but not the abſolute dilatations; becauſe for this purpoſe he ought to have known the capacity of the part of the tube paſſed through by the mercury relative to that of the phial, and to have known exactly the inclination of the tube. Prieſtley ſays nothing of this. I will no longer then diſcuſs his experiments; particularly as Prieſtley himſelf does not place much confidence in them, and wiſhes them to be repeated in a better manner.

but not with great attention:

His reſults as to the increments only.

Suppoſing equal volumes of different gaſes, the expanſions meaſured in inches on the tube would be for 4°, 44 of the thermometer divided into 80 parts.

	Inches.
Common air - - - -	1,32
Hidrogen gas - - - -	2,05
Nitrous gas - - - -	2,02
Carbonic acid gas . - - - -	2,20
Muriatic acid gas - - - -	1,33
Oxigen gas - - - - -	2,21
Azote gas - - - - -	1,65
Sulphureous acid gas - - -	2,37
Fluoric acid gas - - - -	2,83
Ammoniacal gas - - -	4,75

(Exper. and Obſer. &c. Book VII. Sect. VI.)

Vandermonde, Monge, and Berthollet on common air and hidrogen.

In a memoir printed among thoſe of the Academy for the year 1786, Citizens Monge, Berthollet, and Vandermonde, concluded from experiment, that for one degree, atmoſpheric air dilates $\frac{1}{184,83}$ of its volume, and hidrogen gas $\frac{1}{181,02}$.

Guyton and Duvernois.

At length Cit. Guyton conſidering the variations in the reſults on the dilatation of atmoſpheric air, and that there were no direct experiments to determine the dilatation of gaſes at degrees of heat a little elevated or otherwiſe, undertook with Cit. Duvernois to examine this object. As their work is the moſt recent, I ſhall endeavour for a moment to diſcover what may have affected their reſults.

Their experiments.
The gas was extruded from a body by the heat of a bath, and this exceſs received our mercury.

Their apparatus was compoſed of a body with a bended tube, by which means, the air expelled by heat from the body, was received in a jar on the pneumato mercurial apparatus. The body filled with the gas on which the experiment was to be made, was plunged into a bath at the temperature of melting ice, and was kept down by an iron cover. The bath was ſucceſſively heated to the degrees 20, 40, 60, and 80, and they collected in ſeparate jars the products of the dilations for each of theſe degrees. They then determined the volumes of air expelled from the body by meaſuring them in their reſpective jars after having reduced them to the temperature of melting ice, and they determined the volume of that remaining in the body.* But ſetting aſide that their apparatus obliged to aſcertain *many data* which muſt have been an objection to the exactneſs of their reſults, I remark that after the immerſion of the bended tube in the mercury, as they did not introduce new air into the receiver to diſplace the mercury which had entered the tube by reaſon of the preſſure of the mercury in the trough ſeveral degrees of heat muſt be required before any bubble of air can come out; ſo that had they reckoned from diviſions more nearly together, as from 5° to 5° they would have concluded that from zero; the firſt degrees of heat do not cauſe the different gaſes to undergo any dilatation. Hence they found for the firſt twenty degrees a dilatation much too weak for the gaſes in general.

Objection.

The mercury in the tube diſturbed their reſult.

Their retort or body does not appear to have been dry enough.

This cauſe of error, tho' great, would not have made the reſults of Citizens Guyton and Duvernois ſo far from the truth had there not been a ſtill greater. I ſuſpect then that their receiver was not ſufficiently dry, and that a little water might have been introduced with the gas. In fact, a decigramme of water would have been ſufficient to have influenced their reſults very conſiderably, particularly in the higher degrees; for, as it changed into elaſtic fluid, it would have expelled a great quantity of the air from the receivers.

And from this cauſe the expanſions appeared greateſt at the higher temperatures.

The increaſing progreſſion which they obtained in all the gaſes is this way explained, whilſt they ought to have obtained a decreaſe of volume in the products of each dilatation were reduced to the temperature of melting ice.. I ſhall obſerve that Cit. Guyton expreſſes himſelf thus relative to the dilatation of

* Annales de Chimie, Vol. I.

hidrogen gas, (Annales de Chimie, tom, I. page 284.) "The "four products of the dilatation were in this caſe received in "a veſſel which was ſurrounded with another veſſel filled with "ice, Notwithſtanding this, the mercury of the trough in"dicated by the thermometer 2, 3, 4, 6, degrees above zero, "whilſt the water of the bath was at the ſame time at 20, 40, "60 and 80 degrees, which may have occaſioned ſome inac"curacy in the valuation of each of theſe products; but this "cannot be of great conſequence, the dilatation being very "little in theſe firſt degrees."

Hence it may be inferred that theſe philoſophers did not pay more attention to reduce the volumes of the other gaſes to zero; and if that was the caſe, this would be another cauſe of uncertainty in their experiments.

By comparing the volumes of gaſes remaining in the retort with thoſe expelled by heat, Cit. Guyton and Duvernois found that oxigen, hidrogenous and carbonic acid gaſes and atmoſpheric air underwent a diminution, and they attributed the cauſe of it to the combinations which had taken place during the time of performing the experiments. Yet when I uſed very pure mercury, deprived of oxide, I could not obſerve any ſenſible action between this metal and the gaſes from the temperature of melting ice to that of boiling water.

(To be concluded in our next.) p. 257

XIV.

Deſcription of a new Secret Lock. *By* J. B. BERARD.

Preliminary Reflections.

reat utility of cks.

SOME machines are ſo eminently uſeful as to require no diſcuſſion in that reſpect. They are highly intereſting to the whole of ſociety. Locks are of this number. They are a defence ſupplementary to the laws, and a remedy for the conſequences of immorality. From the loweſt to the higheſt ranks in the ſtate, there is no one who does not derive daily advantage from this invention. And accordingly different artiſts

ecret locks;

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* From his Melanges Phyſico Mathematiques. Paris, an 9.

have ſucceſsfully employed themſelves upon them, to the production of ſecret locks, exhibiting different degrees of ingenuity.

But it is not ſufficient that this inſtrument ſhould contain a ſecret difficult to be developed by the ſkilful method of concealment. The proprietor of this lock muſt always have, as partakers with him, not only the maker, but alſo his friends, his domeſtics, and generally every perſon who poſſeſſes a lock conſtructed on ſimilar principles. The queſtion to be ſolved remained as follows:

ought not to admit of partakers in their ſecret.

To invent a lock which cannot be opened but by the proprietor, of which the conſtruction ſhall be ſimple and eaſy, defended againſt all internal derangements, and, laſtly, capable of being readily opened by the proprietor.

Enunciation of the problem of a lock.

By reflecting on the conditions which a perfect lock ought to include, I was naturally led ſome years ago to this reſult; namely, That the ſecret of the lock ought to be variable at pleaſure*, or at leaſt that the ſecrets ſhould be ſo multiplied, that it ſhould be morally impoſſible for the workman who conſtructed the lock, or any other perſon, to conjecture which among them all the proprietor might have choſen at the time. I ſoon afterwards contrived ſeveral kinds of locks founded on this general principle.

Condition that the ſecret ſhould be variable at pleaſure.

By continuing to attend to this object, I ſoon found that others before me had arrived at the ſame reſult, and had publiſhed locks formed on the theory of combinations.

Others as well as the author had purſued this object.

As ſimplicity is one of the moſt eſſential conditions of this purſuit, and there are thouſands of ways of applying the principles of combinations to the conſtruction of locks, I thought it would not be without utility to explain the different reſults I have obtained. For it will not be till after having compoſed the moſt happy combinations, that we can hope to arrive at that ſimplicity, without which a lock of this kind would be more curious than uſeful.

Simplicity an eſſential requiſite.

I ſhall firſt deſcribe the lock which appears to me to be the beſt among thoſe I have thought of. I ſhall afterwards compare it with that invented in 1777, by Cit. Reguier, and de-

The author's lock to be compared with that of Reguier.

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[*] This principle, which is appplied in the well-known lock of Braviali, was explained to me in converſation, much at large, by him, in 1797.

ſcribed as the moſt perfect, in the art of lock-making *(Serrurie)* in the Encyclòpédie Méthodique. I ſhall ſhew that the lock of Reguier, as well as many others, contains an imperfection, or rather a radical defect, which eſcaped the inventor, and renders its other properties of little value *. It will be ſeen how I have avoided this defect in mine. And, laſtly, I ſhall conciſely deſcribe ſome other ſyſtems of locks which ſeem to offer particular advantages, but which, nevertheleſs, do not poſſeſs the ſame degree of perfection as that to which I have given the preference.

Deſcription of the new Lock.

Particular deſcription of the new lock: The front of this lock conſiſts of a plate 230 millimetres † in length, 67 in width, and 4 or 5 thick. Along the middle line are diſpoſed five nuts or ſcrew heads, of 18 in diameter. In the centre of theſe nuts is a ſquare ſtem of 5 in the ſide, which ends in a ſcrew.

This ſquare ſtem carries immediately on the other ſide of the plate, a round plate of 38 in diameter and 3 thick, having a ſquare hole in the center, and having 24 teeth in its circumference, with ſpars between them equal to the teeth.

Upon this round plate is placed a cylindrical cup or ferril, having its bottom in contact with that wheel, and connected with it by the ſtem, which paſſes through a round hole in its center, and binds it down by the ſcrew nut. This ferril, of which the thickneſs is 2 and the height about 29, has a prominent ſtud at its flat face, which entering between any two teeth of the wheel, ſerves to vary at pleaſure the relative poſitions of the wheel and ferril at the time of ſcrewing them together. Laſtly, The ferril has 24 teeth cut in its edge, having a ſpace of about 4 between each. Theſe teeth are about 3 in depth, excepting one of the intervals which is cut to the depth of 6. We ſhall hereafter explain the uſe of this depth.

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* Cit. Reguier has, nevertheleſs, the honour of being the firſt who nearly reſolved this problem.

† To avoid fractions, I have not reduced theſe meaſures. As the metre is 39.371 inches (Philoſ. Journ. II. 250), it will be ſufficiently near if the reader multiplies the numbers in the text by 4, and then cuts off two figures: Thus 230×4=9.20 inches, or 9¼ inches.—N.

The

Conſtruction of a ſecret lock acceſſible only to the proprietor.

The face of the lock is fitted up, as we have obſerved, with five ſcrew heads. The middle ſtem, inſtead of being provided with a wheel and ferril like the others, has a wheel of 45 in diameter, 4 in thickneſs, and has 24 teeth of the depth of 3.

Fig. 1. Pl. II. repreſents the face of the lock. A B is 230, A A 67, and the five ſcrew heads, R, are ſpaced at equal diſtances.

Fig. 2 repreſents the inſide of the lock without its bolt. The plan of the four ferrils with their ſcrews, together with the middle toothed wheel and the heads of the nuts, are viſible, but the four flat wheels are concealed beneath the ferrils.

The ferrils are incloſed in a rectangular box, 2 in thickneſs, and about 28 in height at the longer ſides. The ſhorter are 34 in height, and have an excavation or notch in them to the depth of 8. The ſides of this box are at the diſtance of 6 from the ferrils endwiſe, but only 2 as to the breadth. The diſtance between the middle wheel and the neareſt ferrils is alſo 2, but that between the contiguous ferrils is 5.

In the four inner angles of the box are fixed four pillars, P P P P, in thickneſs 6, in width 13, and about 35 above the plate. Theſe pillars have at their extremities a rectangular groove for the bolt to ſlide in, and they are fixed by ſcrews to the long faces of the box, and by rivets to the end plates. The ſquare frame of the box is faſtened to the face by two cocks Z Z, which receive ſcrews.

The bolt is conſtructed as follows: A thin bar of iron is taken, of which the width C C, Fig. 3, is 67, and the length C D, which is variable towards D, is 310 from F to C. It is fitted with two edge bars C F, C F, of which the width is 5, and another C C, only 4. At the end which is to form the part of the bolt to ſhoot out, a plate F F D D is fixed, of the ſame breadth as C C, but in length arbitrary. The depth of the three edge bars is $6\frac{1}{2}$, and cannot be ſeen in the figure. That of the end plate depends on the ſtrength intended to be given.

The two lateral edge bars C F have near their extremities a projection $x\,x'$, $x''\,x'''$, which exceed the edge bar by 3, and do not come down to the face of the bolt by an interval of 3. In theſe four ſpaces the extremities of the pillar ſlide, and they themſelves receive the projections $x\,x'$, $x''\,x'''$ in their grooves.

Conſtruction of a ſecret lock acceſſible only to the proprietor.

The diſtances of all theſe points from the extremity C C of the bolt are, namely, little $c\,x$ 20, $c\,x'$ 60, $c\,x''$ 226, $c\,x'''$ 274, C F 310.

In order to complete the bolt, it only remains to fix its rack and its four pins correſponding with the four ferrils.

The rack is a rectangle, of which the length is M N, Fig. 5, equal to 67, the breadth or depth $M\,M'$ 33, and the thickneſs, which could not be ſhewn in this figure, is 4, and may be ſeen in Fig. 3. This parallelopepidon is fixed perpendicularly upon the bolt at $M'\,N'$, Fig. 3, by two projecting rivets K K, Fig. 5.

The rack is notched ſo as to form in its length $M'\,N'$, 13 teeth of 5 in depth; ſo that it reſembles a comb with very ſhort teeth. The ſpaces are wider than the teeth, and they are rounded on the ſide which locks into the wheel.

The ſingle tooth or pin which is intended to correſpond to each of the ferrils, is formed of four parts, ſtand up at right angles, as is ſeen in Fig. 4. O H is the pin properly ſo called, rivetted perpendicularly in the bolt by the ſmall ſquare extremity H. I I are two ſmall wedges which precede the tooth. O H is 11, and I I 16.

The diſtance of the tooth or ſtud O from the line C C, Fig. 5, is 39; that of O' is 118; that of O'' is 169; that of O''' 248.

The little wedge which precedes the tooth, is 6 in length and 3 in height. We ſhall hereafter ſee the uſe of this.

From the poſition of theſe four ſtuds it follows, that when they are lodged preciſely in the thickneſs of the ferrils, the two extreme ferrils muſt have their long notch or aperture directly towards the extremities of the lock, and thoſe of the intermediate ferrils will, on the contrary, have their apertures both turned towards the middle wheel. We ſhall ſee alſo that it follows, that when the lock is open, the firſt pin O is out of the lock in the groove cut in the door; the tooth O' is within the ſecond ferrol; the tooth O'' in the ſpace above the wheel; and the tooth O''' within the fourth ferril. And the contrary is the caſe when the door is ſhut.

In order that the bolt may ſtop at the ſame point when the door is open, there is a ſmall protuberance X or X'''. The teeth O, O', O'', O''', are then ſufficiently diſtinct from the ferrils to allow of their being moved or ſet.

And

Conſtruction of a ſecret lock acceſſible only to the proprietor.

And ſo likewiſe in order to ſtop the bolt always at the ſame point when ſhut, there is fixed at Q, near the ſhort edge bar, a ſcrew, the head of which ſtands higher than the face of the bolt, and ſtops it by taking againſt the box. When the bolt is required to be taken out, this ſcrew is drawn back a little, and it paſſes clear of the edge bar. The ſame effect might be obtained by putting an interior ſtem to the handle, which ſhall ſtop the bolt when the door is locked, which takes place when the points X and X″ coincide with the faces of the pillars towards the knob Q. This knob is alſo of uſe to open or bolt the door within. The poſition of the knob Q is ſuch, that the circumference of its ſcrew ſtem is a tangent to the line X X.

The key remains to be deſcribed: This is a circular plate or diſc, Fig. 6, of 50 in diameter and 3 in thickneſs. Its circumference is divided into 24 parts, carrying circularly the 24 letters of the alphabet, leaving out K. It has a handle in the form of a ſemiellipſis, to turn it with the finger and thumb, and this handle carries on the other ſide of the plate two ſmall ſteel points T T, of about 3 in length, which enter into the holes of the ſcrew heads. Laſtly, This plate has an opening in its center about 20 in diameter, to allow the points to be viſible when applied to the ſcrew heads.

There are alſo marked on the face of the lock five ſmall ſtars or lozenges, to ſerve as ſtation-points for the key; and alſo five ſmall holes near one of the two holes of the ſcrew heads. Theſe ſmall holes ſerve as an indication to apply the key always in the ſame manner, by putting the ſame point in the ſame hole.

The poſition of the four ſtars correſponding with the ferrils, muſt be determined with great care and preciſion. For this purpoſe, the ferrils muſt be placed ſo that their openings ſhall be in a right line, and the holes of the ſcrew heads in a vertical line. The bolt is then to be introduced by engaging it in the grooves of the pillars, ſo that the four teeth or pins may be lodged in the thickneſs of the ferrils. The turn-ſcrew is then to be inſerted ſucceſſively in the holes of each ſcrew-head; obſerving that the letter A, having its mark diſtinguiſhed by a diamond, ſhall be turned towards the ſame ſide as the ſmall indicative point placed on the ſcrew-head, and then a point muſt be made on the face of the lock to correſpond with the letter A of the key. At this point the ſtar is to be made, which will

Conſtruction of a ſecret lock acceſſible only to the proprietor.

be very nearly in a vertical line: but it may eaſily be imagined that it will not be exactly ſo, unleſs the ſtuds of the bolt ſhould exactly fill the ſpaces, ſo as to leave no ſhake. The fifth ſtar correſponding to the center wheel, is merely ornamental.

In this diſpoſition of the lock we may affirm, that it is adjuſted to the word A A A A.

In order to change the ſecret, and adjuſt the lock to the word *long*, for example, the firſt ſcrew-head muſt be turned round to the left, until the letter L of the key coincides with the ſtar. In the ſame manner the letter O is made to coincide with the ſtar of the ſecond ſcrew-head; the letter N with that of the third; and that of the letter G with the fourth. The flat wheels remaining in that poſition, the ferrils are then to be diſpoſed in a right line, and the ſecret will conſiſt of the word *long*. The bolt is then to be put in its place, and the handle turned to advance its ſtop.

When the lock is to be ſhut, the ſcrew-heads muſt be diſpoſed ſo as to form the word *long*; and in this poſition the bolt may be moved backward and forward by turning the middle wheel. If one or more of the ſcrew-heads be deranged, the bolt is locked faſt. But in order to open it, the ſame diſpoſition muſt be made, and the bolt may again be moved.

We may obſerve that, when the ſcrew-heads are deranged, the bolt nevertheleſs is capable of moving through the ſpace of 13, ariſing from the ſize of the ferrils; which affords an advantage for uſing it as a ſimple bolt, to be put backwards and forwards within the chamber.

(To be concluded.)

SCIENTIFIC NEWS, &c.

Cheap Method of producing Light.

MR. EZEKIEL WALKER informs me that he has diſcovered a cheap method of producing light, which ſeems to poſſeſs advantages much ſuperior to the common modes of illumination. This light generates no ſmoke, and requires not the aid of ſnuffers.

On

On the Reflection of obſcure Heat.

Pictet's experiments of reflected heat with mirrors.

C. Pictet's experiment related in his eſſay on fire, to prove the reflexibility of obſcure heat is well known. It conſiſts in placing oppoſite to each other two concave metallic mirrors. In the focus of one is placed a hot but not luminous cannon-ball, in the focus of the other a very ſenſible air thermometer, and the latter is ſoon ſeen to riſe rapidly.

The ſame philoſopher has ſince publiſhed ſome other experiments on the ſame ſubject.

Glaſs intercepted the heat but not the light.

Having employed a lighted candle inſtead of the cannon-ball, he placed between the two foci a plate of very thin clear tranſparent glaſs, and which intercepted the light very little: the thermometer indicating the tranſmiſſion of heat, ſtopped that inſtant.

Diſtance of mirrors 25 metres.

The two mirrors were placed at the diſtance of 25 metres (yards) one from the other, in order to determine whether the time of the propagation of the radiant heat from one focus to the other could be appreciated. A heated but not luminous ball was ſuſpended at one of the foci, before which a ſcreen was placed. At the inſtant that this obſtacle was removed, the fluid of the thermometer, which was before perfectly at reſt, began to move, and no ſenſible interval could poſſibly be perceived between the ſuppreſſion and the effects of the tranſmitted heat.

The heat was inſtantly tranſmitted.

C. Pictet relates this experiment in the Bibliothèque Britannique, in ſupport of the opinion he had advanced in his eſſay on fire, that light and heat are not the ſame. This opinion has ſince been renewed by M. Herſchell.—*Bulletin des Sciences, No.* 62.

On certain Facts commonly urged againſt the Doctrine of two Electric Fluids. By Cit. TREMERY, *Engineer of Mines.* (*Bull. des Sciences, No.* 63.)

The experiment of paſſing a ſhock through Lullin's card.

Among the facts which have been offered in ſupport of Franklin's hypotheſis of a ſingle electric fluid, the moſt remarkable is the following, (commonly called *Lullin*'s card, from the inventor of the experiment).

Having placed between two metallic conductors a card, which touches each of them, by its oppoſite faces, in two different points, a ſtrong electric charge is paſſed through the apparatus. At the inſtant of the operation, a luminous flaſh paſſes from the poſitive conductor, ſlides along the ſurface of the card, and perforates it oppoſite the negative conductor. This happens even when the card is perforated beforehand oppoſite the poſitive conductor.

penetrated oppoſite the neg. conductor.

From this fact it has been concluded, that in order to ſupport the theory of two fluids, we muſt ſuppoſe that one of them eſcapes from bodies and produces light, while the other remains inherent *. Cit. Tremery deſtroys this reaſoning by the following experiment:

This does not happen in rarefied air.

He places the card and the two conductors under the receiver of an air pump, and in proportion as the air is exhauſted, the place of fracture happens neareſt the poſitive conductor; and when the air is half exhauſted, the point is preciſely at the middle between the two conductors. At every diſcharge a luminous trace iſſues from each conductor along the ſurface of the card to the place of interſection.

Inferences.

From this experiment, Cit. Tremery concludes that the atmoſpheric air, in its ordinary ſtate, reſiſts the paſſage of the negative more than the poſitive fluid †. Theſe reſiſtances are diſtinguiſhed in different ratios, with regard to the two fluids, as the air becomes leſs denſe, and more rapidly with regard to the negative than the poſitive.

Hence Cit. Tremery deduces the general reſult, that the inſulating property of non-conductors cannot be the ſame for both electricities.

From this explanation, he thinks it eaſy to reconcile the theory of two fluids with the few facts oppoſed to it by his adverſaries.

---

* Or if one ſingle paſs from one conductor to the other, the attraction or power of the receiving conductor will be very oblique to the card at firſt, and moſt direct at laſt; whence the place of fracture is inferred to be at the receiving or minus extremity.

W. N.

† Yet a negative point diſcharges its electricity more eaſily than a poſitive. See Fig. 5, Pl. VIII.

W. N.

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Expansions of the Gases by Heat.

Fig. 1.

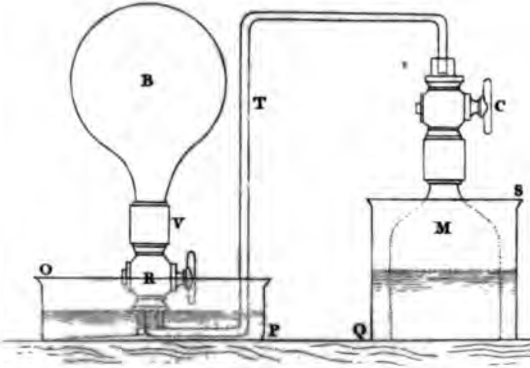
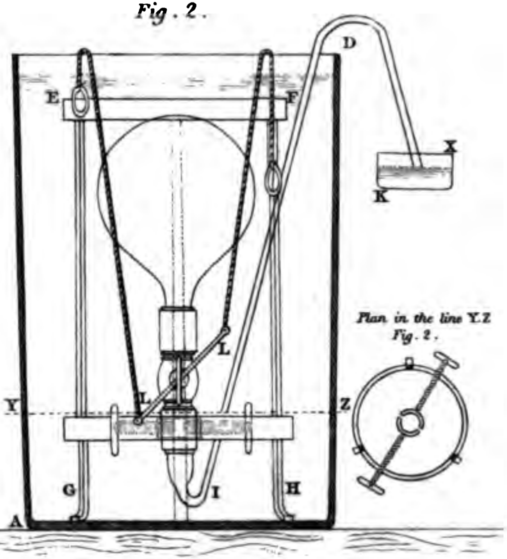


Fig. 2.



Plan in the line Y.Z
Fig. 2.

Mutlow Sc. Russell Ct.

East Indian Hand Mill.

Fig. 1.

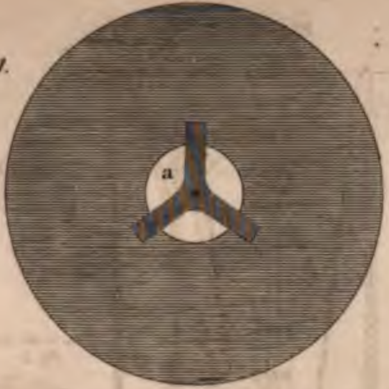
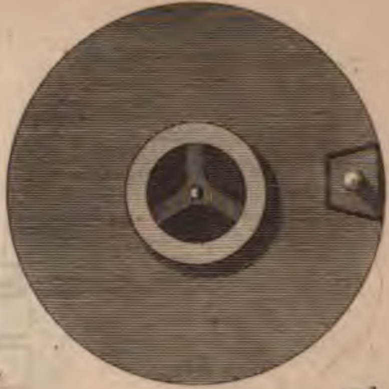


Fig. 2.



Scale 1 inch one foot

Fig. 4.

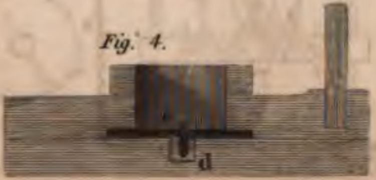
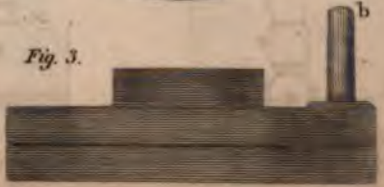


Fig. 3.



Mutlow Sc. Ruſsell Ct.

Secret Lock of Berard.

Fig. 1.

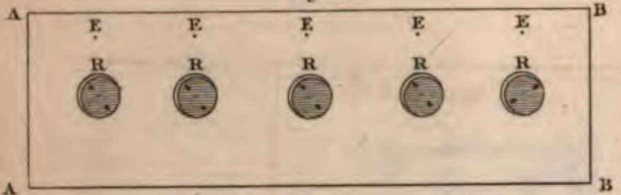


Fig. 2.

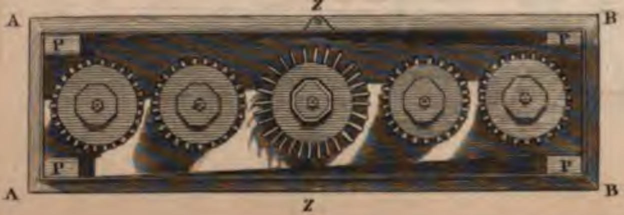


Fig 5.

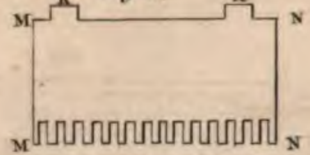


Fig. 6.

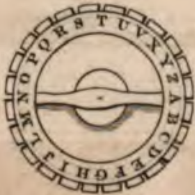


Fig. 7.

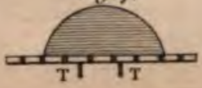


Fig. 8.

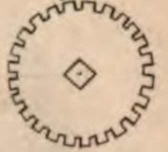


Fig. 9.

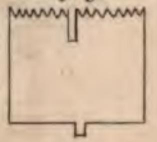
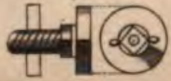


Fig. 10.



Mutlow sc. Russell Ct.

Fig. 3.

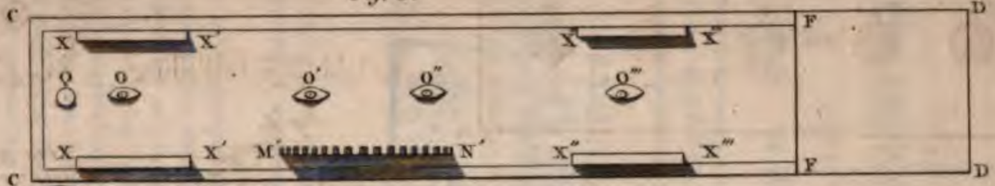


Fig. 11.

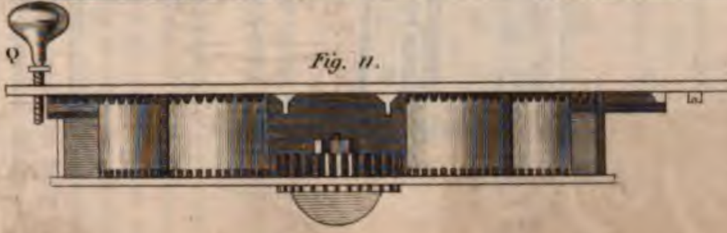
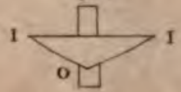


Fig. 4.



Mutlow sc. Russell Ct.

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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

DECEMBER, 1802.

ARTICLE I.

Deſcription of an Improvement of Woulfe's Apparatus, by which the great Inconveniencies of that Method of connecting the Veſſels are obviated. In a Letter from Mr. John Murray.

Edinburgh, Sept. 18, 1802.

To Mr. NICHOLSON.

SIR,

I SEND you a drawing of an improvement in Woulfe's apparatus, which I believe I may recommend with confidence to chemiſts.

In the apparatus on the common conſtruction it is extremely difficult, I might perhaps ſay impoſſible, to have a ſeries of bottles connected by tubes which are ground air tight, and hence the neceſſity of employing luting to cloſe the joinings, an operation extremely troubleſome when done with accuracy. This difficulty of fitting the bent tubes to the bottles by grinding, ariſes from the conſtruction of the apparatus itſelf; for if one leg of the tube be ground into the bottle from which it iſſues, it will be found ſcarcely poſſible to grind the other leg with accuracy into the other bottle, ſo as to fit it at the preciſe

It is ſcarcely poſſible to connect veſſels by air tight tubes without luting.

The cauſe why;

point at which the two bottles ſhall be on a level; and this difficulty is increaſed ſtill more when a ſeries of bottles are to be joined in this manner. The difficulty is indeed ſo great, that an apparatus of this kind perfectly air tight, though it would be of the greateſt advantage in many chemical operations, has perhaps ſcarcely ever been procured, and the attempt to have it made was relinquiſhed even by Lavoiſier.

Luting is moſt commonly uſed.

Various attempts have been made to obviate this inconvenience, but with ſo little ſucceſs, that the original apparatus in which the joinings are cloſed by a lute, is ſtill the one employed by chemiſts. Of the improvements that have been propoſed, I may remark that that by Dr. Hamilton, of which a deſcription is given in his tranſlation of Berthollet on Dyeing, and which does not ſeem to be ſufficiently known to chemiſts, is by far the moſt convenient. It may be ſimplified by having the bent tubes to iſſue from the receivers inſtead of being fitted to them by grinding, and for many purpoſes it anſwers exceedingly well. Its principal deficiency is, that no great degree of preſſure can be obtained in it, proportioned to the quantity of water put into the receivers.

Dr. Hamilton's excellent apparatus.

Cit. Girard's improvement very expenſive, and why:

Cit. Girard has lately propoſed another improvement, of which an account has been given in the 4th Vol. of your Journal (p. 41.) On ordering his apparatus at the glaſs manufactory, I found that it could not be made but at a very great expence. This was ſtated to be owing partly to the trouble which would attend the bending the long tube attached to the bottle, but ſtill more to the difficulty of giving this tube the ſame curvature, as the larger tube fixed in the bottle, into which the former is to be introduced. The execution of this by the mode Cit. Girard deſcribed, would not be ſo eaſy as he appears to have ſuppoſed it; and a number of bottles would probably have required to have been made, to obtain a ſeries of three or four adapted to each other.

This difficulty obviated by the author.

It ſometime afterwards occurred to me, that every difficulty might be obviated by a more ſimple method, which I have ſince found is extremely eaſy in the execution. The apparatus conſtructed on this method is repreſented in the annexed drawing. Plate XIII.

Deſcription. The contrivance of Girard applied perpendi-

A is an adopter ground into the bottle B, and connecting a retort with it. C is a ſtraight tube ground into an opening in the ſide of B, and into a ſimilar opening in the ſide of the bottle D. E is a bent tube paſſing from the neck of D, and fitted to it by grinding; and it is in the mode of connecting this with the ſucceeding bottle G, that the preſent improvement conſiſts. F is merely a common Woulfe's bottle with two necks. But in the firſt neck a tube G is ſoldered, when the bottle is made, which deſcends to within 1½ inch of the bottom, the height of the bottle being ſix inches. The long leg of the tube E deſcends into this tube, reaches beyond the extremity of it, and has at its termination a ſlight curve. It is obvious that when water is put into the bottle F, ſo that the extremity of G is immerſed in it, if any vapour or gas paſs from D, to F by E, it will be diſcharged from the extremity of the latter, and from the degree of bending will be thrown beyond the extremity of G, and as this is immerſed in the fluid, the gas cannot eſcape, but whatever part of it is not abſorbed, muſt be collected in the upper part of F. It paſſes in a ſimilar manner to H, and from H to I, being in each bottle tranſmitted through a portion of water. Any permanent gas, not abſorbed, paſſes through the bent tube fitted to I, and may be collected in a jar placed in a ſmall pneumatic trough.

cularly at the outer part of Woulfe's tubes

The advantages of this apparatus are obvious. All the joinings are air tight without lute, and the tubes are at the ſame time ſo far free, that they are in no danger of being broken by any ſlight agitation or derangement. In making it, it is moſt convenient to have the tubes firſt ground in, and when perfectly dry to have them bent by the blow pipe.

Advantages of the apparatus. It is air tight and ſafe.

The firſt bottle in the apparatus being deſigned to receive any fluid condenſed in it during diſtillation, has no tube of ſafety, becauſe at the commencement at leaſt of the diſtillation, there would be no fluid in which its extremity would be immerſed. On this account it is requiſite that this firſt bottle ſhould be connected with the ſecond by a ſtraight tube, as repreſented in the drawing, and not by a bent tube as in the other bottle, elſe upon any condenſation in the retort or firſt bottle, the liquor would ruſh backward from the ſecond. In this ſecond bottle a tube of ſafety is fitted by grinding, which is neceſſary to guard againſt condenſation. In the other bottle this is not neceſſary, as the open tubes are ſufficient to admit air.

Obſervations.

The only inconvenience in this apparatus is, that the fluid in the bottles is liable to be forced up in the open tubes, in

This apparatus and Girard's a liable to have the fluid preſ upwards between the tubes.

the tube G for example in the bottle F, by the preſſure of the gas accumulated in it from the reſiſtance of the column of water in the other bottles; and this even proceeds ſo far that part of the liquor is forced out entirely. This can in ſome meaſure be obviated by diminiſhing the quantity of fluid in the laſt bottles, or by introducing into them little more than is ſufficient to immerſe the extremities of the tubes. In this way however the preſſure obtained, by which the abſorption of a gas is promoted, is very inconſiderable. The ſame inconvenience is preſent in the apparatus of Cit. Girard, and can be obviated only in the ſame mode. In the preſent apparatus, another improvement may be introduced, by which it is more effectually removed, that of having the ſtraight tubes which are ſoldered into the bottle of a greater length. This is repreſented in the drawing at K L, the open tube A riſing five or ſix inches above the neck of the bottle. It is rather more difficult to ſolder in the tube in this mode, but the advantage from the increaſed height is ſo great, that the apparatus ſhould perhaps always be conſtructed in this manner.

Remedy, by lengthening the external tube.

How to convert a common Woulfe's apparatus into the preſent.

Inſtead of fixing the ſtraight tubes into the bottles when they are made, they might afterwards be fitted in by grinding, and then a common Woulfe's apparatus could be eaſily converted into this improved one. The other mode however is preferable, as we are more certain of having the apparatus air tight, and it is ſo eaſily done that it adds little to the expence.

I am, with reſpect,

your's, &c.

J. MURRAY.

II. *Obſervations*

II.

Obſervations and Experiments upon Oxigenized and Hyperoxigenized Muriatic Acid; and upon ſome Combinations of the Muriatic Acid in its three States. By RICHARD CHENEVIX, *Eſq. F. R. S. M. R. I. A. From the Philoſophical Tranſactions for* 1802.

(Concluded from Page 185.*)*

Third Species. Hyperoxigenized Muriate of Barites.

THE earthy baſes ſeem to follow, in the order of affinities for this acid, at a great diſtance from the alkalis. They are all ſuperſeded by the two juſt mentioned; and it is much more difficult to accompliſh their union with the acid, than is the caſe with potaſh or ſoda. The moſt advantageous method is, to pour warm water upon a large quantity of this earth, procured by Mr. Vauquelin's method; and to cauſe a current of oxigenized muriatic acid to paſs through the liquor, kept warm; ſo that the barites already diſſolved being ſaturated, a freſh portion of it may be taken up by the water, and preſented in a ſtate of great diviſion to the acid. This ſalt is ſoluble in about four parts of cold, and leſs of warm water. It cryſtallizes like the muriate of this earth; and reſembles it ſo much in ſolubility, that I could not ſeparate them effectually by cryſtallization repeated ſeveral times. At firſt, indeed, I deſpaired of ever obtaining any of the earthy hyperoxigenized muriates in a ſtate ſufficiently pure for analyſis. If we conſider them as a genus diſtinct from the alkaline hyperoxigenized muriates, a leading character may be, their great reſemblance to their reſpective ſpecies of earthy muriates. I thought, however, that I might, if not by direct, at leaſt by double affinity, decompoſe the one without the other; and phoſphate of ſilver occurred to me as the moſt likely agent. If phoſphate of ſilver be boiled with muriate of lime, of barites, &c. a double decompoſition enſues; and muriate of ſilver, together with phoſphate of the earth, both inſoluble, are precipitated. To increaſe the action, the phoſphate of ſilver may be diſſolved in a weak acid, ſuch as the acetous; and, though the earthy phoſphate be at firſt retained in ſolution, it will be

Hyperox. mur. of barites.

Soluble; cryſtallizable.

Purification of earthy hyp. ox. muriates; (not by diff. of ſolubility.)

by phoſphate of ſilver, which by double affinity decompoſes the muriates, and leaves the hyperox. muriates in ſolution alone.

The ſilver muſt be free from copper.

ſeparated by expelling the acid. The only condition abſolutely neceſſary is, that the ſilver employed be free from copper. For, in preparing phoſphate of ſilver by phoſphate of ſoda, and by nitrate of ſilver thus impure, copper would be thrown down by the phoſphoric acid; and the phoſphate of copper would be afterwards decompoſed by muriate of lime. Muriate of copper would therefore remain with the earthy hyperoxigenized muriates; or, what is ſtill worſe, a part of the muriatic acid being eaſily expelled from oxide of copper, the hyperoxigenized muriatic acid would be driven off from its baſis, by the more powerful agency of the former. This ſalt has all the properties enumerated as belonging to the genus of hyperoxigenized muriates; and, with heat, the acid is expelled by all acids above the benzoic. I had hoped that, without diſtillation, I could procure the acid from the ſalt by means of ſulphuric acid, which would have left an inſoluble ſalt with barites; but hyperoxigenized muriatic acid is ſo eaſily decompoſed by light, that I have not yet obtained it, to my ſatisfaction, diſengaged and pure. A fact well worthy of attention is that the ſtronger acids diſengage this acid with a flaſh of light, more frequently from the earthy than from the alkaline hyperoxigenized muriates; a phenomenon which, I ſuppoſe, depends upon the relative proportionate affinities, and conſequently the greater rapidity of the diſengagement. But, where all is hypotheſis, it is uſeleſs to draw any inference from a ſingle fact.

Properties of the earthy hyp. ox. mur.

emit light more frequently than thoſe with alk. baſis.

Component parts, hyp. ox. mur. barites.

The proportions of this ſalt are,

Hyperoxigenized muriatic acid - -	47
Barites - - - - - - -	42,2
Water - - - - - - -	10,8
	100,0

Hyp. ox. mur. ſtrontia.

Fourth Species. Hyperoxigenized Muriate of Strontia.

The foregoing obſervations apply to the formation of this ſalt, to the mode of obtaining it pure by phoſphate of ſilver, to its conduct with the acids, to the rank of its acid in the order of affinities, and to its other properties. It is deliqueſcent; and more ſoluble in alcohol than muriate of ſtrontia. It melts in the mouth immediately, and produces cold. Its cryſtals aſſume the ſhape of needles.

Deliqueſcent, and more ſoluble in alcohol than the muriate.

It is compoſed of,

Hyperoxigenized muriatic acid	46
Strontia	26
Water	28
	100.

Comp. parts hyp. ox. mur. of ſtrontia.

Fifth Species. Hyperoxigenized Muriate of Lime.

Hyp. ox. mur. lime very deliqueſcent, and ſoluble in alcohol.

This ſalt is obtained pure, in the ſame manner as the other earthy ſalts. It is extremely deliqueſcent; liquifies at a low heat by means of its water of cryſtallization; and is very ſoluble in alcohol. It produces much cold, and a ſharp bitter taſte in the mouth.

It is compoſed of,

Hyperoxigenized muriatic acid	55,2
Lime	28,3
Water	16,5
	100,0.

Comp. parts hyp. ox. mur. of ſtrontia.

Sixth Species. Hyperoxigenized Muriate of Ammonia.

Hyp. ox. mur. ammonia.

From the property which oxigenized muriatic acid poſſeſſes of decompoſing ammonia, this combination may be thought paradoxical. For, how can an acid much more active than oxigenized muriatic acid exiſt with ammonia, which is deſtroyed by the latter? But this argument may be oppoſed by the ſum of affinities that act in either caſe. If the affinity of compoſition of oxigenized muriatic acid and of ammonia, together with the affinity of oxigenized muriatic acid for ammonia, to form oxigenized muriate of ammonia, be not more powerful than the affinity of oxigen for hidrogen, of azote for caloric, and of muriatic acid for ammonia, the divellent affinities will prevail; and this is what actually happens. But, although oxigen may be held with leſs force of attraction in oxigenized than in hyperoxigenized muriatic acid, yet the affinity of the latter acid for ammonia may increaſe in a much greater ratio, and favour the quieſcent affinities. If carbonate of ammonia be poured into any earthy ſalt of this genus, a double decompoſition takes place; and hyperoxigenized muriate of ammonia is formed. This ſalt is very ſoluble in water, and in alcohol. It is decompoſed at a very low temperature, and

The ox. mur. acid decompoſes ammonia by union of the hidrogen and oxigen; becauſe this laſt attraction, &c. exceeds thoſe which might form ox. mur. of ammonia.

But the hyp. ox. mur. acid does not; becauſe the attraction of compoſ. to form hyp. ox. mur. of ammon. is ſtrongeſt.

This ſalt formed by double affinity: *e. g.* carbonate of amm. + hyp. ox. mur. of earth afford carb. of earth and hyp. ox. mur. of am.

It is very ſoluble in water and alcohol, and decompoſed at low temperature.

Component parts unknown.

and gives out a quantity of gas, together with a ſmell of hyperoxigenized muriatic acid. Such a ſmell is doubtleſs owing to the great quantity of oxigen contained in the acid; it being more than is neceſſary to combine with the quantity of hidrogen contained in the alkali, and therefore ſome of the acid is diſengaged, without decompoſition. All the attempts I have made to aſcertain the proportions of its principles, have been fruitleſs. The formation and exiſtence of this ſalt, as I before ſaid, are very ſtrong proofs of what I have advanced reſpecting the ſtate in which hyperoxigenized muriates at firſt exiſt; and very fully prove the different degree of affinity exerciſed by each acid toward the baſis.

Seventh Species. Hyperoxigenized Muriate of Magneſia.

Hyp. ox. mur. of magneſia.

Its chemical and phyſical properties are nearly the ſame with thoſe of the 5th ſpecies, only that, in addition to the other baſes, lime and ammonia cauſe a precipitate in this ſalt.

Component parts.

Its proportions are,

Hyperoxigenized muriatic acid - -	60
Magneſia - - - - - - -	25,7
Water - - - - - - -	14,3
	100,0

Eighth Species. Hyperoxigenized Muriate of Alumina.

Hyp. ox. mur. of alumina.

It is decompoſed by phoſphate of ſilver,

is deliqueſcent, and ſoluble in alcohol.

I put ſome alumina, precipitated from muriate of alumina, and well waſhed, but ſtill moiſt, into a Woulfe's apparatus, diſpoſed as for the other earths, and ſent a current of oxigenized muriatic acid gas through the liquor. The alumina ſhortly diſappeared; and, upon pouring ſulphuric acid into the liquor, a ſtrong ſmell of hyperoxigenized muriatic acid was perceivable. When I attempted to obtain the ſalt pure, by phoſphate of ſilver, in the uſual way, I found nothing in ſolution but hyperoxigeniſed muriate of ſilver [*]; and all the hyperoxigenized muriate of alumina had been decompoſed. This ſalt, however, appears to be very deliqueſcent, and is ſoluble in alcohol; but I could not aſcertain the proportion of its principles, becauſe I did not obtain it ſufficiently free from the ſimple muriate.

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[*] This ſalt ſhall be particularly mentioned and deſcribed in another part of this Paper. For the preſent, it is ſufficient to ſay, that it is very ſoluble in water; and, in that property, as in many others, is totally different from muriate of ſilver.

Ninth Species. Hyperoxigenized Muriate of Silica.

Hyp. ox. mur. ſilica does not exiſt.

I am inclined to think this ſalt does not really exiſt. A current of oxigenized muriatic acid, ſent through ſome ſilica which had been precipitated from an acid by ammonia, and collected moiſt from the filter, did not ſeem to diſſolve any portion of it. In all barites and ſtrontia, prepared according to Mr. Vauquelin's method, a portion of ſilica from the crucibles is attacked, and taken up, by whatever acid thoſe earths may afterwards be diſſolved in: and, in all potaſh of commerce, there is ſome ſilica; but I have never perceived that any portion of this earth had been diſſolved by this acid.

General obſerv. on the earthy hyp. ox. mur.

The very ſmall portion of earth which, in attempts to form the different ſpecies of this genus of ſalts, is taken up by acids, and the ſtill ſmaller portion of the ſalt ſo formed, which is really in the ſtate of hyperoxigenized muriate, render the operation ſo tedious, that I have confined myſelf to form what was neceſſary to determine their analyſis, in ſuch a manner as I believe to be nearly accurate. It cannot, therefore, be expected that I make myſelf reſponſible, without a right of appeal to further experiments, for the accuracy with which the cryſtalline forms, and other phyſical properties, may have been ſtated. It is impoſſible to obtain ſatisfactory cryſtals from a very ſmall portion of ſalt; and I have attached myſelf more particularly to chemical than to phyſical characters, as being a much more important and certain mode of determination. For the ſame reaſon, I have not examined the combination of the new and rarer earths with this acid. But I do not doubt, that whatever chemiſt undertakes a further inveſtigation of theſe extraordinary bodies, will be amply repaid for his labour.

All muriates loſe ſome acid by ignition.

I have mentioned, in a former part of this Paper, that all muriates loſt a portion of their acid at a red heat. I expoſed one hundred parts of muriate of potaſh, in a crucible, to a red heat, for ſome minutes, and found that they loſt five. I diſſolved them in water, and they manifeſted alkaline properties. Treated by nitrate of ſilver, they gave a precipitate, which ſhewed one per cent. leſs of muriatic acid, than 100 parts

Whence the water of cryſtallization is inaccurately determined.

If the acid of the ſalt be determined by precip. with ſilver before and after ignition, it may be known how much of the loſs by the latter proceſs is acid, and how much water.

parts of the ſame ſalt that had not been expoſed to fire. A violent heat may be neceſſary to expel the laſt portion of water of cryſtallization from certain ſalts, as we know particularly is the caſe with ſulphate of lime. But, if any of the acid can be expelled at the ſame temperature, there is no longer any certainty. The quantity of water, as ſtated by different chemiſts, varies much; and, from ſome experiments I have made, I do not believe it to have been accurately determined. The method I uſed to aſcertain this, was as follows: I expoſed a given quantity of the ſalt to a violent heat, and noted its loſs of weight. I then precipitated, by nitrate of ſilver; and thus knew, how much the quantity of muriatic acid which this ſalt contained, was leſs than that in a like portion which had not been expoſed to heat. I ſubtracted the difference in this quantity, from the total loſs of weight in the ſalt expoſed to heat; and the remainder I conſidered as water. It was upon reſults obtained in this manner, that I founded many of the proportions I have given in this Paper.

Obſervations on elective attractions.

It is ſtated in the tables of Bergman, corrected by Dr. Pearſon, that lime and ſtrontia prefer acetous to arſenic acid. But arſenic acid can expel hyperoxigenized muriatic acid from its baſis, although the acetous cannot act in the ſame manner; therefore, this order of affinities is erroneous. It was not till lately, that we had potaſh and ſoda ſo pure as to be relied upon in delicate experiments; and it is not ſurpriſing that we find miſtakes with regard to their taking the acid from barites, ſtrontia, and lime. But real potaſh and ſoda both precipitate even barites from hyperoxigenized muriatic acid. If ever it becomes eaſy to obtain hyperoxigenized muriate of barites, we may prepare that earth from it in the humid way, and more near to purity, than in the method propoſed by Vauquelin.

Metallic muriatic ſalts.

METALLIC COMBINATIONS OF MURIATIC ACID, IN ITS DIFFERENT STATES.

Hyp. ox. mur. acid diſſolves all *metals*: inflames them and produces muriates.

The action of hyperoxigenized muriatic acid upon metals, is, as may well be expected, rapid, and without diſengagement of gas. It appears to diſſolve every metal, not excepting gold and platina. If the metal be preſented to the acid at the moment when it is diſengaged from the ſalt, inflammation enſues; and the phenomena of light and heat vary according to the metal; but the ſalts thus produced are merely muriates. In order to form real hyperoxigenized muriates, it is neceſſary to take the metal in its fulleſt ſtate of oxidizement, and combine it with the acid, either by double decompoſition, or by paſſing a current of oxigenized muriatic acid gas through the oxide ſuſpended in water. The acid is thus ſeparated into muriatic and hyperoxigenixed muriatic acid; and, in theſe ſtates, combines with the metallic oxide. The metallic hyperoxigenized muriates are different, in every reſpect, from the metallic muriates. Red oxide of iron is diſſolved with difficulty. Oxide of copper more eaſily. Red oxide of lead exhibits the ſame appearances, during its combination with this acid, as with nitric acid. When nitric acid is poured, even in exceſs, upon red oxide of lead, only a part of the oxide is diſſolved, unleſs heat be applied; and what remains becomes a blackiſh brown powder. But, if metallic lead be added, in a juſt proportion, all the red oxide diſappears, and none of the brown powder is formed; neither is there any diſengagement of nitrous gas, when the metallic lead is diſſolvrd. The precipitates cauſed in either caſe, by pouring an alkali into the nitric ſolution, are yellow. Hence it appears, that red oxide of lead contains too much oxigen to be diſſolved by nitric acid. One part of the oxide takes up the exceſs of oxigen, and becomes brown; while the portion which loſes oxigen, becomes yellow, and is ſoluble in nitric acid. The preſence of metallic lead promotes the total ſolution of the red oxide, by taking up the ſuperabundant oxigen. I found that a current of oxigenized muriatic acid gas, like the nitric acid, diſſolved a part of the red oxide, and cauſed the brown powder to be formed, upon which it could not act. Hyperoxigenized muriate of lead is much more ſoluble than muriate of lead; and the acid is very ſlightly attracted by the baſis.

It muſt combine with the maximum oxides, to produce h. ox. muriates.

Theſe greatly differ from muriates.

Habitudes of metallic oxides. Iron. Copper. Lead.

Red oxide of lead is too oxigenated to be wholly diſſolved in nitric acid. Part becomes more oxigenated and the other part diſſolves.

Ox. mur. acid acts ſimilarly.

Hyp. ox. mur. of lead: ſoluble.

But, of all the metallic ſalts formed by the combination of the muriatic acid, in any of its different ſtates, none ſo much deſerve attention as thoſe which have for their baſes, the oxides of mercury. The nature of the ſalts which reſult from the combination of common muriatic acid with the different oxides of this metal, has been ſtated in the moſt contradictory manner, by different chemiſts. But, as the knowledge of hyperoxigenized muriatic acid has thrown ſome light upon the true

Mercurial muriates.

Calomel and corroſ. ſublimate.

true ſtate of calomel and corroſive ſublimate*, I muſt beg leave to dwell at ſome length upon this important part of my ſubject.

It would be uſeleſs to repeat the opinions of the old authors, who have treated of corroſive ſublimate and of calomel. They are to be found in the works of thoſe reſpective chemiſts, and I muſt refer to them for particulars.

Opinions reſpecting corroſive ſublimate.

In the Memoirs of the Academy of Sciences of Paris, for 1780, we find a Paper of Mr. Berthollet, upon the cauſticity of metallic ſalts; in which he appears to think, that the acid in corroſive ſublimate is in the ſtate of what was then called dephlogiſticated marine acid. In 1785, when he had examined the oxigenized muriatic acid with more care, he renounced his former opinion; and gave the reaſons why he no longer adhered to it. Some late experiments of Mr. Prouſt ſhew, that this chemiſt thinks as Mr. Berthollet now does. And theſe may be ranked among the firſt of modern authorities.

Fourcroy conſiders it as hyperox. muriate of mercury;

Notwithſtanding thoſe opinions, Mr. Fourcroy, in his *Syſteme des Connoiſſances chimiques,* ſtill conſiders corroſive ſublimate as a hyperoxigenized muriate of mercury; and deſigns it throughout by that name†. This chemiſt, one of the founders of the methodical nomenclature, is too well acquainted with its principles, to apply the term hyperoxigenized muriate to any thing but a combination of hyperoxigenized muriatic acid. It is evident, therefore, that he conſiders the portion of oxigen, which, in equal quantities of corroſive ſublimate and calomel, is greater in the former, to be combined with the acid, and not with the oxide of mercury. As ſoon as I have ſtated ſome experiments that prove Mr. Fourcroy's opinion to be erroneous, and endeavoured to eſtabliſh the analyſis of corroſive ſublimate and of calomel, I ſhall take notice of a ſalt hitherto unknown, which really is hyperoxigenized muriate of mercury.

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* I regret very much, that I am under the neceſſity of uſing theſe unmeaning terms. But the French nomenclature has made no diſtinction between ſalts formed by metallic oxides in different ſtates of oxidizement, except by the colour, which is an extremely defective and unmeaning method. At all events, this metal is ſo uncomplaiſant as to retain the white colour, in its different oxides combined with muriatic acid. I prefer, however, uſing the old name, to propoſing any proviſional ſubſtitute that might be found defective. This will be farther explained in *Remarks upon Chemical Nomenclature.*

† I have ſaid before, that this acid was talked of by many chemiſts, as if the exiſtence of it had really been proved.

but erroneouſly. For,

I took a portion of corroſive ſublimate, and precipitated by potaſh. The liquor was filtered; and, upon being tried, nothing but muriate of potaſh was found. No re-agent could diſcover the ſmalleſt trace of hyperoxigenized muriatic acid.

Corroſ. ſub; precipitated by potaſh gave muriate of potaſh and no hyp. ox. m. acid.

Sulphuric, nitric, phoſphoric, and many other acids, poured upon corroſive ſublimate, did not diſengage either muriatic, or hyperoxigenized muriatic acid. Nitrate of ſilver, poured into a ſolution of corroſive ſublimate, gave an abundant white precipitate.

The acids diſengaged nothing from it.

From theſe experiments it is evident, that muriatic acid, not hyperoxigenized muriatic acid, is combined with the oxide of mercury in corroſive ſublimate.

Conſeq. muriatic and not h. ox. m. acid exiſts in cor. ſub.

To determine the proportions of this ſalt, I took one hundred parts, and precipitated by nitrate of ſilver. I then took another hundred, and precipitated by potaſh. The reſult of theſe two experiments was ſuch as to eſtabliſh the proportions of corroſive ſublimate as follows:

Component parts by precipitation with potaſh and with nitrate of ſilver.

Oxide of mercury - - - -	82
Muriatic acid - - - - -	18
	100.

But, the acid of this ſalt not being charged with a ſuperabundance of oxigen, we muſt look for the exceſs in the metallic oxide. I took 100 grains of mercury, and diſſolved them in nitric acid; then poured in muriatic acid; and, at a very gentle heat, evaporated to dryneſs. I afterwards ſublimed, in a Florence flaſk, the ſalt that remained, and obtained 143,5 of corroſive ſublimate. But, 143,5 of corroſive ſublimate contain 26 of acid; which will leave 117,5 for the mercurial oxide; and, if 117,5 contain 100 of mercury, 100 of the oxide will contain 85. Therefore, the oxide of mercury in corroſive ſublimate, is oxidized at the rate of 15 per cent.

To meaſure the oxigen, 100 grs. of mercury were diſſolved in nitric acid; mur. acid was then poured in, and the ſalt (cor. ſub.) dried and ſublimed = 143.5; of which 18 per cent. was acid, and conſeq. 15 per cent. of the oxide was oxigen.

Component parts of calomel.

To determine the proportions in calomel, I diſſolved 100 grains of it in nitric acid. The phenomena of the ſolution have been ſo accurately deſcribed by Mr. Berthollet, that I ſhall not repeat them. I precipitated by nitrate of ſilver; and obtained a quantity of muriate of ſilver correſponding with 11,5 of muriatic acid. The oxide of mercury I obtained apart. Therefore, calomel is compoſed of,

Oxide of mercury - - - -	88,5
Muriatic acid - - -	11,5
	100,0.

Calomel was converted into cor. ſubl. by boiling in nitro-mur. acid, evapor. and ſubl. It had gained weight, of which, from the former expts. the proportions of acid and of oxigen were known; whence the proportion of oxigen in the oxide is deduced, viz. 10.7 per cent.

To aſcertain the ſtate of oxidizement of the oxide in calomel, I took 100 grains, and boiled them with nitro-muriatic acid; then evaporated very ſlowly, and ſublimed as above. The calomel was totally converted into corroſive ſublimate, and weighed 113. But 113 of corroſive ſublimate contain 20,3 of muriatic acid, of which, 11,5 were originally in the calomel. The total addition of weight was 13. But the quantity of acid in theſe 13, amounts to 20,3 — 11,5 = 8,8. Therefore, 13 — 8,8 = 4,2, remain for that part of the additional weight which is oxigen. On the other hand, 100 of calomel contain the the ſame quantity of mercury as 113 of corroſive ſublimate, = 79. Theſe 79, with 11,5 of acid, are equal to 90,5, and leave 9,5 for the quantity of oxigen contained in calomel. It would appear, from theſe experiments, that corroſive ſublimate contains 6,5 per cent. more acid, and but 2,8 per cent. more oxigen, than calomel. But this quantity of oxigen is combined with a much greater proportion of mercury; and forms an oxide of a very different degree of oxidizement. For, 88,5 : 9,5 : : 100 : 10,7. Therefore, we may eſtabliſh the following comparative table:—

Comparative ſtatement.

CALOMEL.			CORROSIVE SUBLIMATE.		
The oxide of mercury in calomel is compoſed of,			The oxide of mercury in corroſive ſublimate is compoſed of,		
Mercury - -		89,3	Mercury - -		85
Oxigen - - -		10,7	Oxigen - - -		15
		100,0.			100.
And calomel is compoſed of,			And corroſive ſublimate is compoſed of,		
Mercury 79 Oxigen 9,5	oxide of mercury	88,5	Mercury 69,7 Oxigen 12,3	oxide of mercury	82
Muriatic acid -		11,5	Muriatic acid -		18
		100,0.			100.

Theſe proportions are different from thoſe given by Lemery, Geoffroy, Bergman, &c. But, without calling in queſtion the accuracy and ſkill of theſe chemiſts, it is fair to aſſert, that the pure materials uſed by modern chemiſts, are more likely to lead to ſure reſults, than the impure re-agents of the ancients.

In theſe ſalts we find another inſtance, that, in proportion as metallic oxides contain a greater quantity of oxigen, they require a greater quantity of acid to enter into combination with them.

The moſt oxig. oxides ſaturate the largeſt quantity of acid.

The method I have followed, to aſcertain the proportions juſt ſtated, may appear, at firſt view, not to be the ſhorteſt that I might have adopted. But I have tried others, and I have found none ſo accurate. It is impoſſible, ſynthetically, to convert a given quantity of mercury into calomel, in ſuch a manner as to be certain that none of it is in a different ſtate from that required. And, if we would attack calomel analytically, the action of the alkalies, without which we cannot proceed, is ſuch as to alter the nature of the oxides. I have alſo made many comparative experiments, by diſſolving calomel in nitro-muriatic acid, (which converted it into corroſive ſublimate,) and then precipitating by ammonia; but I have not found theſe trials ſo ſucceſsful as thoſe I have deſcribed. The nature of the precipitate from corroſive ſublimate by ammonia, certainly differs, according to the exceſs of acid that may be preſent; and mercury ſeems to have the power of exiſting in many degrees of combination with oxigen. The only

Obſervations to ſhew that the foregoing proceſſes were foun the moſt accurate and conciſe precaution abſolutely neceſſary, in this mode of operating, is, that while the mercurial ſalt is in an open veſſel, it ſhould not be expoſed to a degree of heat capable of volatilizing any part of it.

London Pharmacopœia preſcribes too much mercury in the formula for cor. ſub. But this is prudent.

The quantity of mercury ordered in the London Pharmacopœia, to convert corroſive ſublimate into calomel, is 9 pounds of mercury for every 12 pounds of corroſive ſublimate. But, from the above experiments, it would appear, that a ſmaller quantity of mercury might ſtrictly anſwer. However, from the reſults of minute inveſtigation, we ſhould not conclude too haſtily upon preparations on the great ſcale; and, I rather think, that the exceſs of mercury ordered by the Pharmacopœia is a uſeful precaution.

In attempts to reduce the mercurial ſalts by other metals, iron failed, zinc threw down mercury, and copper precipitated calomel from cor. ſub.

In my experiments, I attempted to reduce, by means of copper, iron, or zinc, the mercury contained in the mercurial ſalts. Iron did not anſwer the purpoſe: zinc precipitated the mercury a little better; and copper produced a change which I did not expect. If a bit of copper be put into a ſolution of corroſive ſublimate, a white powder ſhortly falls to the bottom; and that powder is calomel. When waſhed, it does not contain an atom of copper, or of corroſive ſublimate.

Calomel is the ſame whether produced in the humid or dry way: and alſo cor. ſub.

Before I conclude theſe conſiderations, I muſt ſay, that whether calomel be prepared in the dry or in the humid way *,

it

Obſervations on Scheele's humid proceſs for calomel. It is not pure, but contains a ſub-nitrate of mercury.

* By the humid way, I do not mean preciſely the method of Scheele. That chemiſt deſires us to boil the acid with the mercury, after they have ceaſed to act upon each other at a low temperature. By this method, the nitric acid takes up an exceſs of mercurial oxide; and the nitrate of mercury thus formed, precipitates by water. Therefore, when this nitrate of mercury is poured into the dilute ſolution of muriate of ſoda, according to the formula of Scheele, the action, on the part of the ſolution, is twofold.

1ſt. The water acts upon one part, and precipitates an oxide, or rather an inſoluble ſub-nitrate of mercury. And,

2dly. A double decompoſition takes place between the nitrate of mercury and the muriate of ſoda. It is with reaſon, that the medical world have ſuppoſed the calomel of Scheele to be different from that prepared in the humid way; for it is, in fact, calomel, plus an inſoluble ſub-nitrate of mercury. In the firſt part of Scheele's proceſs, there is diſengagement of nitrous gas, together with oxidizement and ſolution of ſome of the mercury. When he boils the acid upon the remaining mercury, there is no further diſengagement of

gas;

it does not ſeem to differ chemically; nor does it contain any ſenſible portion of water of cryſtallization. The ſame may be ſaid of corroſive ſublimate.

Hyper-ox. mur of mercury: obtained by paſſing ox. m. gas through water containing red oxide of mercury.

It now remains to ſpeak of the real hyperoxigenized muriate of mercury. I paſſed a current of oxigenized muriatic acid gas through ſome water, in which there was red oxide of mercury *. After a ſhort time, the oxide became of a very dark brown colour; and a ſolution appeared to have taken place. The current was continued for ſome time; and, when I thought that a ſufficient quantity of the oxide had been diſſolved, I ſtopped the operation. The liquor was evaporated to dryneſs; and the ſalt was thus obtained. There evidently was in the maſs a great proportion of corroſive ſublimate, as might be expected, from what I had obſerved to take place in the formation of the other ſalts of this acid; but, by carefully ſeparating the laſt formed cryſtals, I could pick out ſome hyperoxigenized muriate of mercury. I then cryſtallized it over again; and, in this manner, I obtained it nearly pure. This ſalt is more ſoluble than corroſive ſublimate: about four parts of water retain it in ſolution. The ſhape of its cryſtals, I cannot well

It is more ſoluble than cor. ſub. and is ſeparable by cryſtallization.

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gas; yet more mercury is diſſolved. The nitrate of mercury, therefore, rather contains an oxide leſs oxidized after ebullition than before it. The true difference is in the ſub-nitrate of mercury, precipitated, as I before ſaid, by the water in which the muriate of ſoda was diſſolved. And the orange-coloured powder, which remains after an attempt to ſublime Scheele's calomel, is to be attributed to the ſame cauſe. To prepare calomel in the humid way, uniform as to itſelf, and in all reſpects ſimilar to that prepared in the dry way, it is neceſſary, either to uſe the nitric ſolution before it has boiled, or to pour ſome muriatic acid into the ſolution of muriate of ſoda, previouſly to mixing it with the boiled ſolution of nitrate of mercury. In the firſt caſe, no precaution is neceſſary; and, in the latter, the oxide of mercury, which the nitrate of mercury has, by boiling, taken up in exceſs, finds an acid which is ready to ſaturate it. All the mercurial oxide being thus converted into calomel, none of that ſub-nitrate of mercury can be preſent.

Preparation of calomel in the humid way.

The objections made by a medical gentleman againſt Scheele's calomel, when this paper was read before the Royal Society, led me to reconſider the ſubject, and to undertake the inveſtigation detailed in this note.

* I uſed either of the red oxides of mercury indiſcriminately.

determine. When ſulphuric, or even weaker acids, are poured upon it, it gives out the uſual ſmell of hyperoxigenized muriatic acid; and the liquor becomes of an orange colour. This is a ſufficient proof, that corroſive ſublimate is not a hyperoxigenized muriate of mercury.

Dark brown oxide of mercury not taken up, ſuppoſed to be different from the red oxide.

I have juſt mentioned that, in the formation of this ſalt, the oxide of mercury, which was not diſſolved by the acid, became of a very dark brown colour. I procured a portion of this oxide, which ſeemed different from the red oxide. It however retained the form, and the cryſtalline appearance, of the latter. It was ſoluble in nitric acid, without diſengagement of gas; and was precipitated from it, in a yellow oxide, by all the alkalis, except ammonia. It formed corroſive ſublimate with muriatic acid; and the precipitate by the alkalis, was the ſame as that from corroſive ſublimate, made with the red oxide. Yet I am inclined to think, that the dark brown oxide differs in ſome eſſential point from the red; but I have not yet made ſufficient experiments to prove this opinion. At all events, the preſent object being to examine the mercurial oxides only as combined with muriatic acid, it would be foreign to the purpoſe, to enter into too minute an inveſtigation of the other ſtates of the metal. This, and ſome other objects hinted at in this Paper, muſt be reſerved for future inquiry.

Hyper-ox. mur. of ſilver

In treating the earthy hyperoxigenized muriates with phoſphate of ſilver, as I mentioned before, I obſerved that the liquor ſometimes contained in ſolution oxide of ſilver; which, upon examination, I found to be combined with hyperoxigenized muriatic acid. As the ſalt which is thus formed is different, in every reſpect, from ſimple muriate of ſilver, it may be of ſome importance to conſider it with attention. In the firſt place, it will afford the moſt convincing proof of the difference between muriatic and hyperoxigenized muriatic acid; and, in the next place, it particularly deſerves to be remarked, for poſſeſſing, in the moſt eminent degree, one of the great characteriſtic features of the genus to which it belongs.

ſoluble in 2 parts warm water, and cryſtall. by cold:

Hyperoxigenized muriate of ſilver is ſoluble in about two parts of warm water; but, by cooling, it cryſtallizes in the ſhape of ſmall rhomboids, opaque and dull, like nitrate of lead or of barytes.

ſlightly ſol. in alcohol.

It is ſomewhat ſoluble in alcohol. Muriatic acid decompoſes it; as does nitric, and even acetous acid: but the reſult of this decompoſition is not, as might be expected, nitrate or acetite of ſilver. At the moment that the acid is expelled from hyperoxigenized muriate of ſilver, a re-action takes place among its elements: oxigen is diſengaged; and the muriatic acid remains in combination with the oxide of ſilver. If this fact be compared with the manner in which nitric and acetous acids act upon hyperoxigenized muriate of potaſh, it will give a ſtrong proof of the proportionate affinities of all theſe acids for oxide of ſilver, in compariſon with that which they exerciſe towards the alkali.

Acids decompoſe it, but leave muriate inſtead of ſeizing the ſilver.

Hyperoxigenized muriate of ſilver, when expoſed to a very moderate heat, begins by melting, and then gives out a conſiderable quantity of oxigen gas, with effervescence; and muriate of ſilver remains behind. Theſe phenomena however differ much, according to the degree of heat applied. When hyperoxigenized muriate of ſilver is mixed with about half its weight of ſulphur, it detonates in the moſt violent manner; and does not, like hyperoxigenized muriate of potaſh, require the addition of charcoal, to poſſeſs a very great force of exploſion. The ſlighteſt preſſure is ſufficient to cauſe this mixture to detonate; and I think I ſhall be within bounds, when I ſtate, that half a grain of hyperoxigenized muriate of ſilver, with a quarter of a grain of ſulphur, explodes with a violence at leaſt equal to five grains of hyperoxigenized muriate of potaſh, with the due quantities of ſulphur and charcoal. The flaſh is white and vivid, and is accompanied by a ſharp and quick noiſe, like the fulminating ſilver ſo ably deſcribed by Mr. Howard; and the ſilver is reduced to the metallic ſtate, and vaporized.

Hyp. ox. m. of ſilver loſes oxigen by heat, and becomes muriate.

With ſulphur it explodes; by the ſlighteſt preſſure, at leaſt ten times as ſtrongly as hyp. ox. mur. of potaſh.

I think it right to add a few remarks upon what I have termed the proportionate affinities of acids and of baſes, one for the other. It is a law, not indeed univerſally, but frequently obſerved, and very well worthy of conſideration, that the acids are attracted by metallic oxides, in a very different order from that in which they are diſpoſed to unite to alkaline and earthy baſes.

Order of attraction of acids for oxides is frequently very different from that with alkalis.

Nitric acid, which holds ſo high a place in the order of affinities for alkalis, is expelled from metallic oxides by moſt acids. Phoſphoric, fluoric, all the vegetable acids, except two or three, and the animal acids, attract the latter baſes more ſtrongly. Nay, we ſhall find, upon an attentive examination, that

Acids commonly attract oxides more the leſs they act on the metals:

that acids commonly attract metallic oxides, in the inverſe ratio of their action upon metals, or, in other words, in proportion to their own affinity of compoſition. Thus, the phoſphoric and fluoric acids ſometimes rank before the ſulphuric; and the nitric, as I before ſaid, is generally very low. Hyperoxigenized muriatic acid ſeems to follow the ſame rule; and takes its place, in the order of affinities for metallic oxides, after many of thoſe acids which it can expel from earths and alkalis.

Hyp. ox. m. acid follows this rule.

The metallic butters have their metal and not their acid extraordinarily oxigenized.

The other hyperoxigenized muriates, I have not yet ſufficiently examined. I ſhall, however, mention at preſent, that I have aſcertained the muriatic ſalts, formerly known by the ſtrange name of *butters of the metals*, to be muriates, and not hyperoxigenized muriates; and the extraordinary proportion of oxigen, to be combined, not in the acid, but in the metallic oxide.

In the courſe of different experiments, I have known hyperoxigenized muriatic acid to be formed in two caſes, where I could not have expected it.

Precipitation of oxide of titanium from mur. acid by potaſh, gave oxigen to the latter, and formed ox. m. potaſh.

In the analyſis of ſome menachanite from Botany Bay, given to me laſt year by the Preſident of the Royal Society, I obſerved, that while the oxide of titanium was precipitated from the muriatic acid in which it was diſſolved, the exceſs of oxigen in the oxide paſſed over to the muriatic acid and the potaſh, already in the liquor, and that hyperoxigenized muriate of potaſh was formed. I have attempted the ſame experiment with black oxide of manganeſe, but could not ſucceed.

When nitro-muriatic acid is diſtilled from platina, *the metal is oxided, and ox. and hyper-ox. m. acid are formed.*

There is, however, a ſtill more extraordinary formation of this acid, in the diſtillation of nitro-muriatic acid upon platina. Oxigen is abſorbed by the metal; yet, not only oxigenized, but alſo hyperoxigenized muriatic acid is formed. I have repeated the experiment ſeveral times; and am well convinced of the fact, however contrary to theory it may appear. I have tried the action of oxigenized muriatic acid, upon nitric acid, in the hopes of forming hyperoxigenized muriatic acid; but there was no action to this effect among their elements.

This fact imperfectly ſeen before.

The fact of the production of a peculiar gas, by the diſtillation of nitro-muriatic acid upon platina, has been obſerved by

Mr. Davy, in his *Reſearches*[*]. But, as hyperoxigenized muriatic acid was not known at that time, he could not ſay the real nature of that gas. Had Mr. Davy carried his ingenious experiments a little farther, we ſhould have been much earlier acquainted with the laſt degree of oxigenizement of muriatic acid.

Berthollet conſi- ders muriatic acid as a radical &c.

Mr. Berthollet terminates his Paper upon hyperoxigenized muriate of potaſh, by ſaying, that he will conſider muriatic acid as the radical; oxigenized muriatic acid, as correſponding with ſulphureous and nitrous acid; and the acid which he conjectured to exiſt in this ſalt, as correſponding with ſulphuric and nitric acid. I ſhall now conclude, by ſtating the arguments in favour of each denomination, and the analogies upon which they are founded.

The arguments ſtated on this ſubject.

Muriatic acid is for us a ſimple body; but it has acid properties of the ſtrongeſt kind; therefore, from analogy, we ſuppoſe it to contain oxigen. But may not this be too haſty a concluſion? Are we not very doubtful concerning the exiſtence of oxigen in pruſſic acid? And are we not, on the contrary, certain that ſulphurated hidrogen, which poſſeſſes many of the characteriſtics of acids, does not contain any? Of the oxigenizement of fluoric and boracic acids, we have no proof: but then we cannot affirm that any one of theſe acids exiſts in three ſtates of combination with oxigen; and the muriatic is the only radical of which we admit this fact. We muſt not, however, pretend to limit the number or degrees of combinations between combuſtible bodies and oxigen; but we can ſpeak with certainty only of thoſe things which are proved. Beſides its acid properties, this ſubſtance has others, common to oxigenizable bodies. With 16 of oxigen, it forms an acid, which, in many of its properties, is to its radical what the ſulphureous is to ſulphur. Like the ſulphureous, it is volatile; has little attraction for ſalifiable baſes; deſtroys vegetable blues; and is capable of further oxigenizement. With 65 of oxigen, it becomes more fixed, like ſulphuric acid; has a ſtronger affinity for ſalifiable baſes; and acquires more truly acid properties. Upon theſe conſiderations, I ſubmit to

Mur. acid, like ſome others, h the properties oxigenizable bodies, and may not contain oxigen.

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[*] Dr. Prieſtley, alſo, mentions a peculiar gas, produced by diſtilling a ſolution of gold in *aqua regia*.

the chemical world, whether, in the preſent ſtate of our knowledge, it be not more philoſophical to ſay,

Nomenclature of this hypotheſis.

Muriatic radical, or ſome ſingle word of the ſame import,	inſtead of	Muriatic acid;
Muriatous acid,		Oxigenized muriatic acid;
Muriatic acid,		Hyperoxigenized muriatic acid,

It is not proved that every acid contains oxigen.

I am fully aware that, at firſt ſight, this may appear extraordinary; and the more ſo, as we have no poſitive facts that prove muriatic acid to be a ſimple body. All we can, therefore, conſider fairly, is, in favour of which appellation does the ſum of analogies ſeem to preponderate. And, to give the cauſe a candid inveſtigation, we ſhould begin by conſidering, whether the preſence of oxigen in all bodies that have acid properties, has been rigidly demonſtrated; and not determine by this law of the French chemiſtry, till we are well convinced it has not been too generally aſſumed.

Nomenclature ſhould be governed by the facts; and not the contrary.

If a nomenclature be not ſubſervient to the uſes of ſcience, and does not keep pace with its progreſs, the relation between ſubſtances and their names will become ſo relaxed, that confuſion will be brought about, by the very means we take to avoid it; and if, while we continue to extend our acquaintance with chemical bodies, nomenclature remains confined within its former limits, the bonds that unite theſe two parts of the ſcience muſt inevitably be broken.

III.

On the preſent State of the Britiſh Muſeum. By a Correſpondent[*].

The Britiſh Muſeum ſtated to be of no public utility.

IT will probably be conſidered a preſumption, even bordering on impertinence, to remark, that no eſtabliſhment is of leſs utility to the public, and none a greater reproach to the nation, than the Britiſh Muſeum in its preſent ſtate; but as the remark is made with the pureſt motive, it is hoped its apparent petulance will be excuſed.

That it is not open to the public in any beneficial manner;

That this magnificent and valuable collection, bought by the public for their own uſe, and entertained at their own expence,

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[*] Whoſe name accompanied the paper.—W. N.

pence, ſhould be rigorouſly ſhut up from the public themſelves, is ſo extraordinary and diſgraceful a fact, that I am wholly at a loſs to account for it, particularly when I reflect that the fact occurs in a nation who prides itſelf, above all things, on the free enjoyment of its liberty and its property.

It may be objected to me, that the Muſeum is not literally ſhut up from the public, and that, on the contrary, it is in the power of any individual to procure a ſight of it. True; it is in the power of any one to procure a curſory view of it, after the formality of inſcribing his name on the regiſter of admiſſion, and waiting until the keepers think proper to appoint him a preciſe day and hour, when it is very likely his other engagements may deprive him of the poſſibility of attending. But, ſuppoſe the appointment procured, and the time convenient, what gratification or inſtruction can be derived from prowling through a ſuite of apartments, at the heels of a keeper, who is anxious to diſpatch you as ſoon as poſſible, and in company of a dozen people, each of whom is eager to catch a tranſient glance of every object within his reach, and each exclaiming at the "wondrous ſurpriſing" things which ſurround him.

because the times of inſpection are not made convenient to ſtudents;

and the actual proceſs is that of a multitude hurried through a ſuite of rooms.

The library, the moſt valuable and moſt extenſively uſeful part of the Britiſh Muſeum, is preciſely that part which is moſt completely ſecluded from the public. To viſit this it is indiſpenſably neceſſary to be introduced to the librarian, and to obtain *his* permiſſion to conſult the collection. How different is it on the Continent! there, ſcarce a town of any note but has its public library, to which free acceſs may be had at all times, by every individual, whether ſtranger or native. How humiliating the compariſon! and how ſingular that London, where the ſciences and the arts are cultivated with greater ſucceſs probably than in any other ſpot of the globe, ſhould be deſtitute of a ſingle public library.

The library *ſaid to be* acceſſible only at the pleaſure of the librarian.

Not ſo on the Continent.

Reflection.

If I might preſume to ſuggeſt a plan for rendering the Britiſh Muſeum a real benefit to the public, I would propoſe that the library, and the collection of objects relative to natural hiſtory, &c. ſhould be formed into two ſeparate eſtabliſhments, to each of which the public might have the freeſt acceſs during as great a portion of the day as poſſible.

Suggeſtion; that the library and collection ſhould be ſeparated.

The library might be open all the year round, from eight in the morning until four in the afternoon, and again from ſix until ten at night. It ſhould be furniſhed with tables, pens,

Regulations for the library.

and ink: the floor ſhould be matted, and the room properly heated during the winter ſeaſon. The books ſhould be delivered by the aſſiſtant librarians to thoſe who might require them; but on no account ſhould any book be permitted to be taken out of the library. A regular regiſter might be kept of the books delivered out each day, with the names of thoſe who required them; and the delivery might be cancelled as they were returned to the librarian. It might perhaps be proper, on account of the value of the manuſcripts, to require the expreſs permiſſion of the principal librarian for their peruſal.

Concerning the expences and other objects.

A proper fund ſhould be provided for the neceſſary expence of the library, which might be under the direction of a committee: ſuch ſums as were over and above the expence of the maintenance of the library, might be applied to the purchaſe of ſuch books as the committee ſhould ſee expedient. But to contract, as far as poſſible, the expence of ſupplying the library with new publications, the donations of public ſpirited individuals ſhould be ſolicited, and the names of the donors regiſtered in a conſpicuous manner on each book they preſented. The committee might be empowered to ſell, by public auction, from time to time, ſuch duplicates as might be received, &c. and be thought unneceſſary to retain in the library. The accounts of the library might be adjuſted every quarter; but they ſhould conſtantly remain open for the inſpection of the public. A regular catalogue of the library ſhould be printed in octavo; and ſupplements might be added from time to time, as the new acquiſitions ſhould render it neceſſary: one or more of theſe catalogues ſhould be conſtantly kept in the library, for the purpoſe of general reference. A binder and proper aſſiſtants ſhould be attached to the library, through whoſe hands each volume ſhould paſs at leaſt once every year; and it might not be beneath the attention of the committee, to hold out premiums for any improvements which ſhould be made in the binding of books, ſo as more effectually to preſerve them from the attack of the worm, and from gradual decay.

The collection of natural hiſtory. Direction:

The collection of objects relative to natural hiſtory, &c. ſhould be under the ſuperintendance of a ſeparate committee; and this eſtabliſhment, if properly conducted, could hardly fail to become one of the fineſt and moſt inſtructive repoſitories in Europe. No nation is ſo well ſituated for eſtabliſhing a national Muſeum as our own: our extenſive commerce, and the public ſpirit of individuals, would ſhortly ſupply us with the choiceſt ſpecimens from every quarter of the globe; and at this very moment thouſands of valuable ſpecimens are rotting in obſcurity, for want of a national Muſeum to receive them.

Arrangement of the collection.

Five claſſes.

Animals; vegetables; minerals; curioſities; objects of art.

The arrangement of this eſtabliſhment requires conſiderable knowledge and ability; it would therefore be the higheſt preſumption in me to attempt to dictate on the ſubject. I ſhould conceive, however, that the whole collection might be advantageouſly ſeparated into five claſſes. The firſt might comprehend the animal, the ſecond the vegetable, and the third the mineral kingdom; the fourth might comprehend objects of pure curioſity, and ſuch as were illuſtrative of the ſtate of ſociety, of arts, and of manufactures of various nations; the fifth might compriſe ſuch objects as tended to the improvement of, or were ſimply illuſtrative of the arts, ſciences, and manufactures of our own country.

Natural hiſtory. Subjects.

Catalogue.

In the arrangement of the objects of natural hiſtory, I would ſuggeſt that the moſt extenſive ſcale ſhould be aſſumed, and that ſpaces ſhould be left for the inſertion of ſuch objects as were wanting, and were likely to be procured: the places of theſe objects ſhould be ſupplied by accurate drawings or models, if poſſible. To each object might be attached its proper name, both in Latin and in Engliſh; and a deſcriptive catalogue of the Muſeum ſhould be printed, which might at the ſame time ſerve as an introduction to the ſtudy of each claſs.

Donations.

The donations of public ſpirited individuals ſhould be ſolicited for this eſtabliſhment, as for the library; and the names of the donors ſhould be regularly inſerted in the public prints. The committee ſhould alſo be empowered to diſpoſe of ſuch duplicate ſpecimens as might be ſent, by public auction.

Laboratory.

A laboratory for the preparation of objects of natural hiſtory ſhould be annexed to the eſtabliſhment; and the committee might be directed to offer premiums for improvements in the art of preparing and preſerving theſe objects. The committee might alſo be enjoined to publiſh conciſe directions for travellers, for collecting, preſerving, and tranſporting the different objects they might meet with; copies of this work ſhould be given to all who might requeſt them.

Conſequences of ſuch an extenſive and liberal

I am inclined to think that if this eſtabliſhment was conducted on an extenſive and a liberal ſcale, it would ſhortly compriſe

moſt

conduct of this eſtabliſhment.

moſt of the private collections in the kingdom; for it is preſumed that there are few lovers of natural hiſtory who would not prefer the honourable applauſe of completing the national Muſeum, to the ſelfiſh pleaſure of poſſeſſing an imperfect and comparatively uſeleſs collection of their own, eſpecially as they would enjoy the credit of the collection equally as if it remained in their own private poſſeſſion.

Extenſion of the Muſeum.

By degrees as the collection enlarged, and as its utility was more ſenſibly felt, it is to be preſumed that the nation might be induced to purchaſe ground, and to erect appropriate and extenſive buildings to contain it: in ſhort it would ſeem, were this plan to be adopted, that we might indulge ourſelves with the proſpect of ſpeedily poſſeſſing one of the fineſt, the moſt intereſting, and moſt inſtructive eſtabliſhments in the univerſe.

J. G.

ANNOTATION. W. N.

Public eſtabliſhments are of great importance.

They have vigour at firſt;

but ſoon decay.

Is this degeneracy inevitable?

Endowments, &c.

Inſtances. Greſham College and others.

Though the importance and value of public eſtabliſhments have been ſtrongly felt and admitted in all ages, yet there has not, as far as I know, been any direct inveſtigation of the means of giving them permanence and effect. A public library, college, inſtitution, or ſociety, may be radically defective in its conſtitution, and, nevertheleſs, flouriſh for a time by virtue of the ability and active exertions of its founders; and it is certain, that every conſtitution whatever will afford only a periſhable eſtabliſhment, unleſs it contain ſome inducement by which ſuch exertions ſhall be invited and continued in its favour. It appears to be extremely difficult, and perhaps in ſtrictneſs impoſſible, to produce motives of this kind by mere regulation; and truſtees, managers, inſpectors, or governors, being bound by the letter of the ſtatutes, can ſeldom be expected to make advances in ſupport of the ſpirit of the foundation. Is it an unavoidable conſequence, that ſchools, univerſities, and ſocieties, ſhall degenerate into mere inſtruments for conferring degrees or titles? Are all eſtabliſhments periſhable unleſs endowed? Is it from the operation of inevitable cauſes that various endowments are productive of little or no effect, and that moſt unendowed eſtabliſhments diſappear in a few years? Among the former we may mention the Greſham foundation and various public libraries in this town, with ſeveral colleges, ſchools, and ſocieties, which it would anſwer no good purpoſe to name without more ample comment; and among the latter the colleges of Warrington and Hackney, begun with great ſpirit, but in a few years abandoned. Or may we not rather hope, that an inveſtigation of conſtitutional and hiſtorical facts, reſpecting all the public means of inſtruction in this and the neighbouring countries, would aſſiſt us in the means of giving full energy to theſe important eſtabliſhments? How are the collections and libraries on the Continent regulated as to ſupply of materials, preſervation from injury or embezzlement, and ready acceſs by the ſtudent? What may be the ſtate of the muſeums and libraries in London, and our univerſities, in theſe and other reſpects? Can the collective power of any body of men be eminently applied to forward literature and the ſciences; and if ſo, why has it generally failed? &c. Theſe and many other intereſting ſubjects of inquiry, might be cleared up by Correſpondents of Journals, who may not have leiſure for the direct and ample diſcuſſion of a ſubject of ſuch magnitude and extent.

The hiſtory of theſe objects may probably ſhew how to preſerve their energy.

Correſpondents invited to ſtate the orders of the continental libraries, the Britiſh collections, &c. in order to make progreſs.

IV.

Of the Properties of the Earth Yttria, *compared with thoſe of* Glucine; *of Foſſils, in which the firſt of theſe Earths is contained; and of the Diſcovery of a new Subſtance of a metallic Nature (Tantalium). By* A. G. EKEBERG *.

THE firſt part of this Memoir contains the particulars of the experiments made by M. Ekeberg, to confirm the difference exiſting between glucine and the earth laſt diſcovered in gadolinite, and named yttria or gadoline; but as the particular nature of each of theſe earths is well known to chemiſts, it would be ſuperfluous to ſay any thing in this place on the ſubject.

First part of the Memoir.

* *Kongl. ſwænſk. Vetenſkaps, academiens nya Handlingar*; *fser ær* 1802, *Qvartal I. Pag.* 68.

This abridged Memoir is from the Journal de Van Mons, but I take it from the Journal de Phyſique, LV. 281.

As there is novelty to recommend the ſecond part of this Memoir, we ſhall communicate it to our readers in a faithful tranſlation.

Mineral ſubſtance newly inveſtigated, containing a metal hitherto unknown.

The author remarks, that although the mineral ſubſtance which he has diſcovered contains yttria, it cannot be claſſed in a ſyſtem of mineralogy as a ſpecies of that earth, on account of the more abundant preſence of another equally remarkable ſubſtance, which muſt be carried to the numerous claſs of metals. He found this ſubſtance in two foſſils obtained from different ſituations: in one place it was united with iron and mangane ſe, and in the other with iron and gadoline.

Inſoluble in all the acids; ſoluble by fuſion in alkali;

precipitable by acid, but not ſoluble;

the precipitate white, even after ignition;

This new metallic ſubſtance is characterized by its inſolubility in all the acids. The only re-agent that has any action on it, is fixed cauſtic alkali. When expoſed to the fire with that alkali, the maſs being afterwards lixiviated, it partly diſſolves in water, and may be precipitated from this ſolution by means of an acid, but the precipitate is not in the leaſt attacked, however great the quantity of acid employed. When ſeparated by filtration and dried, it has the form of an extremely white powder, which does not change colour even at a red heat. The remainder of the maſs being treated with acids, affords the ſame powder. Its ſpecific gravity, after ignition, is 6,500. It is fuſible with the blow-pipe, by the addition of alkaline phoſphate and borate of ſoda, but it does not communicate any colour to the flux,

ſoluble by heat with alkaline phoſphate or borate; but gives no colour.

It is reducible to a metallic button by charcoal;

and is then oxidable, but not ſoluble by acids.

It appears to be a new metal;

When expoſed to a ſtrong heat in a crucible, without any mixture except pounded charcoal, it is reduced to a moderately hard button, having a metallic brilliancy at its ſurface, but its fracture is dull and blackiſh. The acids have no other action on this kind of regulus, than that of cauſing it to return to the ſtate of white oxide. The circumſtances of the reduction, as well as the ſpecific gravity of this ſingular ſubſtance, ſeem to place it among the metals, and I have ſufficient reaſon to be perſuaded that it is not the ſame as any of thoſe hitherto known. The ſubſtances with which it is likely to be confounded are the oxides of tin, of tungſten, and of titanium, which are ſoluble in the cauſtic alkalis, and in all circumſtances reſiſt the action of acids. But the oxide of tin is eaſy to be diſſolved and reduced; tungſten is eaſily detected by its ſolubility in ammonia, and by the blue colour it communicates

not tin, becauſe inſoluble, and not, like it, reducible;

nor tungſten, becauſe it is neito phoſphate of ſoda; the oxide of titanium gives to borax the colour of hyacinth, and becomes ſoluble in acids when fuſed with carbonate of potaſh *.

ther ſoluble in ammonia, or gives a blue colour to phoſ. ſoda; nor titanium, becauſe it does not colour borax, nor become ſoluble in acids when fuſed with carb. potaſh.

External characters.

Before the author deſcribes the chemical analyſis he has undertaken of theſe two ſubſtances, which he regards as ores of the new metal, he has choſen to deſcribe their external characters. In order to avoid circumlocution, he has ventured to give them a generic name. By availing himſelf of the uſage which admits of mythological denominations, and in order to expreſs the want of power of this new metal to become ſaturated with the acids into which it is plunged, he has given it the name of *tantalium;* for the ore compoſed of tantalium, of iron, and of manganeſe, he propoſes the name of *tantalite;* and for that containing yttria, the appellation of *yttrotantalite,* which cannot be thought more heavy than that of ſiderotitanium, already adopted.

The new metal denominated tantalium.

its ores tantalite and yttrotantalite.

Locality and gangue, &c. of tantalite.

He received the ſpecimen of tantalite from M. Geyer, Director of Mines, who has aſſured him that this ſubſtance has been known ſince the year 1746, and has been regarded as a problematical variety of tin garnet *(zinngraupen)*. It is found near the farm Brokaern, in the pariſh of Rimito, in the government of Abo, in Finland, in a large mountain, on the banks of the Baltic. The gangue is compoſed of white quartz and mica, with veins of red feld ſpar in large plates; ſubſtances which compoſe the ſides of the gangue. The titanite is diſſeminated in the form of garnets.

Size and figure of cryſtals.

The ſpecimens he has ſeen were in detached cryſtals, of the ſize of a nut, the moſt regular among which ſeemed to approach the octahedral form: they contained particles of feld ſpar and mica.

Surface.

Their ſurface is even, poliſhed, and blackiſh.

Fracture.

Fracture compact, and of a metallic brilliancy; colour of the fracture not alike all through; it varies between a greyiſh blue and the black of iron.

Powder; grey browniſh.

When pulverized, it is of a blackiſh grey approaching to brown.

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* The author nevertheleſs entertained ſome ſuſpicions of the identity of this new ſubſtance with the latter metal, and nothing but a compariſon with the titaneous iron of Norway, which he decompoſed with this view, convinced him that this identity does not exiſt.

Gives ſparks. It is ſufficiently hard to give ſparks with the ſteel.

He did not remark whether it is attracted by the magnet.

Sp. gravity. Its ſpecific gravity is 7,953.

Locality and gangue, &c. of yttrotantalite. The yttrotantalite is found in the ſame place and in the ſame gangue as gadolinite. Klaproth ſays, that the latter is found incruſted in a maſs of granite; but though the conſtituent parts of granite are occaſionally found with it, it is not the leſs inſiſted that feldſpar is its true gangue, as is clearly ſeen in the inſtances of the great quarry at Ytterby. The mica and quartz found in it form diſtinct parts, and do not enter into combination with the feldſpar; (he ſpeaks only of the maſſes of the principal rock, without diſputing the poſſibility of particles of the three ſubſtances being found mixed); but in general it is a rock of feldſpar interſected by large veins of mica in a direction almoſt perpendicular; and the gadolinite and yttrotantalite are to be ſought near theſe veins. The firſt is commonly found attached to one ſide of a vein of ſilvery mica, and the reſt of its maſs enveloped with feldſpar. The ſecond is never found immediately adherent to mica. The maſſes or kidnies which it forms, are enveloped with a thin cruſt of feldſpar, ſeparated from the general maſs by ſlight layers of a greyiſh black mica. The gangues charged with theſe kidnies are ſeldom inſulated: They are found in cluſters, ſeparated from the principal rock and from one another by ſimilar coatings of mica. Theſe ſubſtances are generally thus diſpoſed by nature, and it is very ſeldom that they are found diſſeminated in grains in the rock of feldſpar.

Size and fracture of the maſſes. The largeſt maſſes he has found of the ore of yttrotantalium were not of the ſize of a nut. Its fracture is granulated, of the black colour of iron, with a metallic brilliancy.

Hardneſs. Its hardneſs is not conſiderable; it may be ſcraped with a knife, though difficultly.

Powder. When in powder it is of a greyiſh colour.

Not magnetical. The magnet does not attract it.

Sp. gr. Its ſpecific gravity was found to be 5,130; but as no ſpecimen was found totally free from feldſpar, it was ſuſpected that its real weight is more conſiderable.

On gadolinite. The abridger concludes his extract by giving an outline of that part of M. Ekeberg's Memoir, which treats of the analyſis of gadolinite and the compariſon of yttria with glucine, which ſeveral writers have affirmed to be of the ſame nature.

Gadolinite when pure is hard enough to give fire with ſteel.

It is hard;

It is found cryſtallized in an imperfect manner like ſome ſpecies of garnets.

cryſtallized;

It contains glucine.

contains glucine.

Beſides the diſtinctions between gadoline and glucine eſtabliſhed by Klaproth and Vauquelin, theſe earths differ in the following peculiar properties.

Gadoline and glucine differ

The ſpecific gravity of gadoline differs conſiderably from that of glucine, which is only 2,967, whilſt that of gadoline is 4,842. This laſt earth is the heavieſt of all the known earthy ſubſtances, ſince it is 0,842 heavier than barites, which only weighs 4,000.

in ſpec. gravity.

It is ſoluble in the alkaline carbonates.

It is abſolutely inſoluble in the cauſtic alkalis.

It is not precipitated by the ſuccinates as glucine is.

Gadoline is ſoluble in alkaline carbonates.
not in pure alk.
nor (like glucine) precip. by ſuccinates.

V.

On the Probability that the Eruptions of Lunar Volcanos may reach the Earth. By J. B.

To Mr. NICHOLSON.

SIR,

Cambridge, Nov. 6, 1802.

THE fact of ſtony and metalline ſubſtances having at different times fallen on the earth, ſeems to be confirmed by ſuch ſtrong evidence, that a doubt can ſcarcely remain as to its reality; permit me through the medium of your Journal, to offer to your readers an attempt to account for this phenomenon. The exiſtence of lunar volcanos, at leaſt equal in magnitude to thoſe of the earth, has been proved by ocular demonſtration. The velocity with which a body muſt be projected from the moon's ſurface ſo as to be carried within the ſphere of the earth's attraction, is about 12,000 feet per ſecond; now when we conſider the height to which volcanic ſubſtances are projected during an eruption, and how very conſiderable a part of ſwift motion muſt neceſſarily have been deſtroyed by the reſiſtance of the atmoſphere, I think it by no means improbable, that the force exerted may be conſiderably greater than would be neceſſary to carry a body 12,000 feet in

Stones fallen on the earth.

Lunar volcanos.

Velocity of a projectile to be thrown from the moon.

vacuo.

The lunar atmoſphere reſiſts little.

Whence bodies may be thrown to the earth.

vacuo. The atmoſphere of the moon cannot offer ſo great reſiſtance to the motion of bodies as that of the earth, as it is certainly much rarer than ours, and probably its height is not much greater than that of the lunar mountains. From all theſe circumſtances taken together, I think it by no means improbable that the volcanoes of the moon may occaſionally project bodies with ſufficient force to carry them to the earth.

I remain, SIR,
your's,
J. B.

OBSERVATIONS.—W. N.

Remarks on the above letter;

with other very looſe conjectures.

Againſt the conjecture of J. B. it ſeems to be a principal difficulty that the ſtones fallen on the earth are thought to have arrived in a ſtate of ignition, which they muſt have preſerved through their immenſe paſſage from the moon. If the meteors or ignited globes be allowed to be ſtones of the kind in queſtion, their extreme velocity in ſome inſtances may be thought to favor his hypotheſis, But this ſubject, like every other in which the data are few, affords wide range for ſpeculation. When we reaſon *from our want of knowledge*, there is ſcarcely any branch of ſcience more favorable to the proceſs than chemiſtry. We are ignorant of the component parts of the earths, the metals, oxigen, azote, hidrogen, &c. and of the very exiſtence of light, caloric, electricity, and magnetiſm, as independent bodies. Some, if not all of the former may be, and moſt probably are, compounded. We ſee very ſudden or inſtantaneous ſeparations of water, light and electricity from the atmoſphere. May not iron and nickel be among its ingredients, and may they not by cauſes equally unknown, be no leſs rapidly ſeparated, with the emiſſion of light and heat? Even if theſe metals exiſt in the atmoſphere diſſeminated, ſuſpended, or diſſolved like aqueous vapors, (and we know they emit effluvia perceptible to the ſmell) their quantity may elude all chemical teſts from its minuteneſs, and ſtill be ſufficient to anſwer the phenomena. A cubic foot of air does not weigh 100 grains, and if one thouſandth part of this were metal, the quantity contained in the air to the height of 500 feet over a ſingle field of ten acres would be about a ton and a half, of which a ſmall part being precipitated, would afford a prodigious ſhower of ſtones.

VI.

Enquiries concerning the Dilatation of the Gaſes and Vapors. Read to the National Inſtitute of France. By CIT. GAY LUSSAC.

(Concluded from Page 216.)

THE following is the table of reſults of Citizen Guyton and Duvernois, in which they have placed between brackets thoſe in which they place little confidence.

	From 0° to 20°	From 20° to 40°	From 40° to 60°	From 60° to 80°	From 0° to 80°
Common air dilates to	$\frac{1}{12,67}$	$\frac{1}{5,61}$	$\frac{1}{2,49}$	$\left(\frac{1}{3,57}\right)$	$\frac{1}{1,067}$
Vital air	$\frac{1}{22,12}$	$\frac{1}{4,92}$	$\frac{1}{1,53}$	$\left(3+\frac{1}{1,73}\right)$	$4+\frac{1}{2,09}$
Azote gas	$\frac{1}{29,41}$	$\frac{1}{5,41}$	$\frac{1}{1,82}$	$5+\frac{1}{57,2}$	$5+\frac{1}{1,062}$
Hidrogen gas	$\frac{1}{11,91}$	$\frac{1}{6,92}$	$\left(\frac{1}{6,85}\right)$	$\left(\frac{1}{58,82}\right)$	$\frac{1}{2,55}$
Nitrous gas	$\frac{1}{15,33}$	$\frac{1}{9,00}$	$\frac{1}{3,739}$	$\left(\frac{1}{6,88}\right)$	$\frac{1}{1,65}$
Carbonic acid gas	$\frac{1}{9,049}$	$\frac{1}{5,099}$	$\frac{1}{2,31}$	$\left(\frac{1}{3,69}\right)$	$1+\frac{1}{106,3}$
Ammoniacal gas	$\frac{1}{3,58}$	$\frac{1}{1,75}$	$1+\frac{1}{1,35}$	$\left(3+\frac{1}{4,69}\right)$	$5+\frac{1}{1,248}$

Before I proceed further, I ought to premiſe, that though I had frequently found that oxigen, azote, hidrogen, carbonic acid gas, and atmoſpheric air, dilate equally from 0° to 80°, Cit. Charles had remarked the ſame properties in theſe gaſes fifteen years ago; but having never publiſhed his reſults, it was by mere chance that I became acquainted with them. He likewiſe endeavoured to determine the dilatation of gaſes which are ſoluble in water, and he found in each a particula dilatation differing from thoſe of the other gaſes. In this reſpect my experiments differ much from his.

Apparatus of Cit. Charles. A barometer with a large vacuum, and a reſervoir below for the gas to preſs on the mercury.

The apparatus of Cit. Charles conſiſted of a barometer whoſe chamber or upper ſpace was of great extent. The gas on which the experiment was to be performed was encloſed in the reſervoir, or lower ſpace of the barometer, at the temperature 0°, and under the preſſure of 28 inches of mercury. When the barometer was plunged in boiling water, the mercury roſe in the tube, and the exceſs of the whole column above 28 inches indicates the elaſticity acquired by the gas; but Cit. Charles having had the politeneſs to ſhew me his apparatus, I ſaw that the tube of the barometer was too large in proportion to the capacity of the reſervoir; ſo that the elevation of the mercury above 28 inches did not indicate all the ſpring the gas had acquired, becauſe in that caſe its volume in the reſervoir ought to have remained conſtant. It appears then to me, that the real dilatation of the gaſes cannot be decided from his experiments.

This was inaccurate, becauſe the riſe of the mercury gave room for expanſion.

ART. III. *Deſcription of Apparatus.*

Apparatus for the gaſes. A globe with an iron cock; filled as uſual, and immerſed in water to be heated. The gas is ſuffered to expand and paſs out, and the expanſion meaſured.

A receiver B (Fig. 1. Pl. IX.) has a cock of iron R, to which may be adjuſted a bended tube I, D, (Fig. 2). The key of the cock carries a lever L L bored at its two extremities in order to receive two cords, by means of which the cock may be opened and ſhut under the water. In order to introduce the gas into the receiver I uſe a bell glaſs M, (Fig. 1.) to which are adapted a cock and bended tube T, plunged in a veſſel Q, S. When water is poured into the veſſel, and the cock opened, the gas compreſſed in the bell eſcapes through the tube and fills the receiver B, reverſed on a bath P, O, of mercury. The receiver B being full I ſhut the cock: I adjuſt the tube I, D, (Fig. 2.) to it, and I fix it in a cylindrical cage of iron E, F, G, H, which I then carry to a copper veſſel A, D, filled with water. In order that there might be no communication between the exterior air and the gas incloſed in the receiver, when the cock is opened I cauſe the extremities of the tube I, D, of one or two millimetres (thirtieths of an inch) to be plunged in a ſmall bath K, X, of mercury. This done I heat the bath, and from 10° to 10° for example, I open the cock and ſhut it immediately. The gas dilated by the heat coming briſkly out of the receiver, quickly drove off the atmoſpheric air that filled the tube, and after the 40° the cock may be ſafely left open during the remainder of the operation: I prefer however alternately opening and ſhutting it, becauſe I find that the gas of the receiver acquires the temperature of the bath better. After 15 or 20 minutes of ebullition, a time ſufficient for the whole to acquire the ſame temperature, I diſengage the extremity of the tube I, D, from the mercury in order to re-eſtabliſh the equilibrium of preſſure between the exterior air and the gas of the receiver, and I then cloſe the cock. After having cooled the bath with ice or water I take away the apparatus, I diſengage the receiver from which I take the tube I, D, and the lever L, L, and I plunge it altogether in a bath of a known temperature, where I leave it a ſufficient time to acquire its temperature.

The relative bulks of the gas at different temperatures determined by weighing.

When the cock is opened a volume of water enters the receiver, which when reduced to a level is preciſely equal to that of the gas expelled by the heat. When the cock is ſhut I withdraw the receiver; I dry its exterior ſurface with care, and I weigh it in this ſtate. I then weigh it firſt full of water, and then empty, and note the reſults of each weight. By this knowledge I obtain the capacity of the receiver by ſubtracting its weight when empty, from its weight when filled with water; and alſo the volume of the water which repreſents the volume of air expelled by the heat from the receiver, by ſubtracting again the weight of the receiver when empty, from its weight when containing this water. Hence it is very eaſy to determine the proportion of the firſt volume of the air to that of the dilated.

This method is very exact.

This method has the advantage of being very exact; for as the volumes are determined from the weights, any error that is committed in this determination muſt be very trifling, even if the ſcales employed ſhould be but moderately ſenſible.

Another apparatus nearly as exact and more eaſily conſtructed.

The apparatus I have juſt deſcribed is ſimple enough in itſelf; but as it contains cement, and a cock which ought to be made of iron on account of the mercury, it is rather difficult to execute. It will not therefore be amiſs to deſcribe another apparatus which I employ, and which on account of its great ſimplicity and eaſy execution, very nearly combines all the advantages of the former.

A body with a long neck; graduated,

plunged in mercury;

It is a ſimple globe or body, and the neck of which muſt be at leaſt one decimetre (about four inches) long. After having filled it with the gas by the method already deſcribed, I plunge its neck about two centimetres (three quarters of an inch) in mercury contained in a common glaſs veſſel, and I ſecure it by an iron frame like the preceding apparatus. If I were to plunge it in this ſtate into a bath of heated water, the dilated gas in order to eſcape would have to overcome not only the preſſure of the mercury in the glaſs, but likewiſe that of the water of the bath.

A ſmall tube to communicate with the outer air when requiſite.

To remedy this inconvenience, I introduce into the neck of the receiver the extremity of a very fine bended tube, taking the precaution to keep the upper aperture cloſed till the tube is plunged beneath the mercury. To ſupport the tube I tie a ſtring about its middle, to the end of which I ſuſpend a weight, and I paſs it over a ſupport in ſuch a manner, that the cord by its action ſhall have a tendency to draw the tube upwards. The apparatus being thus diſpoſed I plunge it in a glaſs veſſel, the water in which muſt be on a level with that in the bath: I open the extremity of the tube for an inſtant, that the equilibrium of preſſure with the external air may be reſtored, and I then ſhut it.

Obſervation of the mercury in the neck.

As there is a ſcale marked on the neck of the receiver whoſe diviſions are extremely minute, I find exactly the level or ſtation of the mercury in the neck, and I note it, becauſe it is at this ſtation that the capacity of the receiver terminates. The lower extremity of the tube muſt riſe clearly above the ſurface of the mercury, which otherwiſe would flow into the tube and reſiſt the paſſage of the dilated gas. After all theſe operations, which are longer in the deſcription than the execution,

When the apparatus is plunged in boiling water, the expanded gas eſcapes by the tube which is then withdrawn.

I carry the apparatus to a bath of hot water, and I open the upper extremity of the tube, after having plunged it into a ſmall bath of mercury, as in the preceding arrangement. When the receiver is at the temperature of boiling water I withdraw the tube, the extremity of which muſt be previouſly diſengaged from the mercury, and I cool the bath.

Water is ſuffered to enter as it cools,

The mercury then riſes into the receiver; but it is eaſy to ſubſtitute water as the temperature diminiſhes.

and the expanſion determined by weight.

The capacity of the receiver, and the volume of water which replaces the gas expelled by heat, is determined in the manner already deſcribed; excepting that in this determination to the weight of the empty receiver muſt be added that of a cylinder of water terminated at one end by the line of the ſtation of the mercury, and at the other by the neck of the receiver.

I might add a few more details, but I ſhall ſuppreſs them for the ſake of brevity, and becauſe perſons very little accuſtomed to manipulation will eaſily ſupply the defect.

However, as it is important, from what I have already ſaid to exclude water entirely from the apparatus, I ſhall mention how I ſucceeded in completely effecting this purpoſe.

Methods of excluding moiſture from the gas.

If the receiver was viſibly moiſt, I firſt dried it with blotting paper, and then heated it ſo as to evaporate part of the water it might ſtill contain, and by means of bellows, to which I adjuſted a glaſs tube, I expelled the vapour by a current of air. Theſe laſt operations being repeated ſeveral times on the receiver and tube, both were found perfectly dry. The mercury which I uſed in my experiments was always employed very dry and pure.

The terms of temperature in the following experiments were freezing and boiling water.

In all the experiments, the reſults of which I am about to give, I always reduced the gaſes (the dilatation of which could be determined by the apparatus here deſcribed) to the temperature of melting ice. For this purpoſe I had a bath with ice in it, into which the receiver was wholly immerſed, after having withdrawn it from the bath where it was placed for the experiment, and was left there for about half an hour, during which I often ſtirred the ice. The other fixed temperature, at which I reſted for the ſame gaſes, was that of boiling water.

Experiments at other temperatures require to be repeated.

I made ſome experiments at other temperatures; but they require to be again repeated, and will beſides form a part of a work I have begun on the law of the dilatation of gaſes and vapors. I ſhall therefore confine myſelf to the dilatation of gaſes at a fixed elevation of temperature, which ſhall be that compriſed between the degree of melting ice and the boiling point. With regard to the vapors, I ſhall compare their dilatation with that of the gaſes.

ART. IV. *Experiments and Reſults.*

Expanſion from freezing to boiling water.

When I uſed the two apparatuſes here deſcribed, but oftener the ſecond than the firſt, and avoiding all the cauſes of uncertainty

Of atmoſpheric air,

uncertainty that I could ſuſpect, I obtained the following ſix reſults from ſix experiments on atmoſpheric air *.

From the temperature of melting ice to that of boiling water, equal volumes of atmoſpheric air repreſented by 100, became - - -
137,40
137,61
137,44
137,55
137,48
137,57

† The mean reſult is about - - - 137,50

for each degree,

If the total augmentation of volume be divided by the number of degrees which produced it, or by 80, it will be found, by making the volume at the temperature 0 equal to unity, that the augmentation of volume for each degree is $\frac{1}{213.33}$, or $\frac{1}{266.66}$ for each degree of the centigrade thermometer.

This reſult does not agree with Deluc.

As Deluc obtained $\frac{1}{215}$ for his coefficient, it ſeems at firſt ſight that our reſults are the ſame; but if it be obſerved that he began at the temperature 16°¼, and that I began at 0°¼ it will be ſeen that our reſults are conſiderably different. I ſhall explain this difference elſewhere, and ſhew that the coefficients of dilatation vary according to the temperature of commencement.

Expanſion of hidrogen;

Hidrogen gas obtained from iron by weak ſulphuric acid was ſubmitted to two experiments: in one, by an elevation of temperature from the degree of melting ice to that of boiling water, 100 parts became 137,49; and in another by the ſame elevation of temperature 100 parts became 137,66. The mean of theſe two reſults is 137,52, which differs very little from the mean reſult of the dilatation of atmoſpheric air.

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* My receiver contained about 350 grammes of water (a little more than 12 ounces)

† Though the difference between theſe reſults are not very conſiderable, I think I might have rendered them leſs ſo, if I could have kept an account of the ſtate of the barometer at the moment of the ebullition of the water. I have, however, always had the precaution to be aſſured of its thermometrical ſtate at the moment of ebullition, in which I can aſſert that I never found any very ſenſible variations. A variation of an inch of the barometer would in fact be neceſſary to occaſion one, of a degree, in the place of the boiling point; which can ſeldom happen. However this may be, this mean reſult 137,50 muſt be very near the truth.

—oxigen,

Oxigen gas obtained from oxigenated muriate of potaſh was dilated three times, and gave the following reſults.

100 parts became - - - 137,47
137,54
137,45

The mean reſult of which is - - - 137,48

—azote.

Azote gas obtained from the decompoſition of ammonia by oxigenated muriatic acid gave the following mean reſults.

100 parts became - - - 137,42
137,56
137,50
137,46
137,55

The mean reſult is - - - - 137,49

Tabulated reſults of four gaſes ſhew the ſame expanſion.

By connecting the preceding reſults, and by comparing the dilatation of oxigen, hidrogen, and azote gaſes with that of atmoſpheric air, we obtain the following table:

From the temperature of melting ice to that of boiling water, 100 parts of	Increaſe their volume to	Differences
	Parts	
Atmoſpheric air -	37,50	
Hidrogen gas - -	37,52	+ 0,02
Oxigen gas - -	37,48	− 0,02
Azote gas - - -	37,49	− 0,01

The trifling differences obſerved in the preceding reſults may proceed from the impoſſibility of rendering the circumſtances rigorouſly the ſame in each experiment; and as they extend to only two ten thouſandth parts of the original volume, it may be concluded with ſafety, that the dilatation of atmoſpheric air, oxigen, hidrogen, and azote gaſes are the ſame from the temperature of melting ice to that of boiling water.

Apparatus for gaſes ſoluble in water. They were compared by expoſure to heat over mercury in a tube, while atmoſpheric air was ſimilarly treated.

In order to determine the dilatation of gaſes ſoluble in water I changed the apparatus, I uſed two tubes graduated at the ſame time on the ſame bath of mercury with a very ſmall meaſure. Every time I uſed this apparatus, I took the precaution that the quantity of mercury ſhould be the ſame as when the tubes were graduated. I muſt even obſerve, that if any accident happens to the veſſel containing the mercury, the tubes muſt be again graduated on another bath. It would even be proper to cut them out of the ſame tube of glaſs and to make them of equal heights, ſo as to render all the circumſtances as ſimilar as poſſible.

The tubes with the two elaſtic fluids were heated in a ſtove.

Into one of theſe tubes I put atmoſpheric air to the 100th diviſion, for example, and in the other the gas on which the experiment was to be made likewiſe up to the 100th diviſion. 100 equal meaſures of theſe gaſes were thus ſubmitted to experiment. I then carried the apparatus into a ſtove, the temperature of which I regulated at pleaſure, and I obſerved the courſe of the dilatation of the gaſes. Whatever attention I paid to the obſervation I never could perceive any difference, and I always remarked that the ſame diviſions were paſſed through in the ſame time.

The gaſes were purified and dried before they were tried.

The gaſes thus examined were never directly received in the tubes; but they previouſly remained ſome time in an intermediate veſſel into which I introduced ſome deſiccative body, muriate of lime for example, and I then paſſed them into the tubes, compreſſing them with mercury, which I poured by means of a tube of ſafety adapted to the intermediate veſſel. If theſe general precautions be neglected the dilatation will almoſt always be too great; becauſe the influence of undiſſolved water will not be guarded againſt, or of ſome other body capable of eaſily taking the gaſeous form.

Carbonic acid gas;

One hundred meaſures of carbonic acid gas obtained from marble by means of ſulphuric acid, were compared with the like portion of atmoſpheric air. From the 5th to the 90th degree (Reaumur) the expanſions were the ſame.

and muriatic acid gas;

One hundred meaſures of muriatic acid gas obtained by means of concentrated ſulphuric acid from muriate of ſoda (very dry) being compared with 100 meaſures of atmoſpheric air from the 3d to the 86th degree, the expanſions of the gaſes were found to be abſolutely the ſame. This experiment, as well as the preceding, were repeated ſeveral times, and the ſame reſults were always obtained.

and ſulphureous and nitrous gaſes expand preciſely the ſame as atmoſpheric air.

Sulphureous acid gas and nitrous gas undergo the ſame dilatation by the action of heat as atmoſpheric air.

Prieſtley, Guyton, and Duvernois found ammoniacal gas expand more

Doctor Prieſtley, and Citizens Guyton and Duvernois, found that ammoniacal gas dilated very conſiderably. With a view to diſcover the cauſe that could have induced them to draw this concluſion from their experiments, I introduced (without preparation) into one of my tubes ammoniacal gas, obtained from the decompoſition of muriate of ammonia by common lime, and in the other tube an equal volume of atmoſpheric air. The ammoniacal gas dilated progreſſively more than the atmoſpheric air in proportion to the elevation of temperature; ſo much ſo that its volume was ſoon double that of the atmoſpheric air, but on obſerving the ſurface of the mercury, and the ſides of the tube after the temperature was lowered, I remarked ſome traces of a liquid and ſome cryſtalline ſpecks, which could be nothing but muriate or carbonate of ammonia, and it all diſappeared when the temperature was ſufficiently elevated. I performed this experiment again by ſuffering the ammoniacal gas to remain ſome time in an intermediate veſſel containing cauſtic potaſh, and then its dilatations from 0 to 95° exactly followed that of the atmoſpheric air. In this experiment I obſerved the ſurface of the mercury and the ſides of the tubes when the temperature had returned to 0, but I could not perceive any indication of liquid, or any cryſtalline ſpeck. This experiment was repeated ſeveral times, and always with the ſame event.

than common air.

becauſe water and a neutral ammoniacal ſalt were preſent.

Hence it may be ſeen that not only a liquid, but another body capable of taking the gaſeous ſtate, may render the experiment liable to error. Theſe cauſes ought therefore to be avoided with the moſt ſcrupulous attention.

Water is not therefore the only cauſe of error.

The experiments which I have juſt related, and were all made with great care, inconteſtibly prove that atmoſpheric air, oxigen, hidrogen, azote, nitrous, ammoniacal, muriatic acid, ſulphureous acid, and carbonic acid gaſes expand equally by the ſame degrees of heat; and that, conſequently, their greater or leſs denſity under the ſame preſſure, and at the ſame temperature, their more or leſs conſiderable ſolubility in water, and their particular nature, in no reſpect influence their dilatation.

Hence the different qualities of the gaſes do not affect their expanſion by heat.

I therefore conclude that gaſes in general, provided they be all placed in the ſame circumſtances, dilate equally with equal degrees of heat.

Theſe experiments on the dilatation of gaſes naturally led me to examine that of vapors; but ſuſpecting from the preceding reſults that they dilated like the gaſes, I determined to make experiments on one vapor only, and I choſe in preference that of ſulphuric ether, as being the moſt eaſy to manage.

Vapors.

In

The vapor of ether was tried in the tube; in compariſon with atmoſph. air.

They expanded alike between 60 and 100° Reaumur.

When near its boiling point the ethereal vapor was condenſed rather more.

In order to determine the expanſion of the vapor of ether I employed the two tubes I have already mentioned, atmoſpheric air being always the ſtandard of compariſon. This apparatus having been kept ſome time in a ſtove whoſe temperature was about 60°, I introduced ethereal vapor into one of the tubes, and into the other atmoſpheric air, ſo that they each correſponded to the ſame diviſion. I then elevated the temperature of the ſtove from 60° to 100°, and I had the ſatisfaction to find, that whether the temperature was raiſed or lowered, the ether and the atmoſpheric air always correſponded at the ſame time to the ſame diviſions. This experiment, at which Citizen Berthollet was preſent, was repeated ſeveral times, and I never could obſerve any difference in the dilatation compared with that of atmoſpheric air. I remarked, however, that at ſome degrees above the point of the ebullition of ether, its condenſations were a little more rapid than thoſe of atmoſpheric air. This is owing to a phenomenon which a great number of bodies preſent in paſſing from the liquid to the ſolid ſtate, but which does not preſent itſelf a few degrees above that of this period.

The concluſion may therefore be extended to vapors;

This experiment, by proving that the vapor of ether and the gaſes dilate equally, ſhews us that this property in no reſpect depends on the peculiar nature of gaſes and vapors, but only on their elaſtic ſtate, and we muſt conſequently conclude that all gaſes and vapors dilate equally at the ſame degrees of heat.

The vapors will be more compreſſible near their point of denſe fluidity or liquefaction.

Since all the gaſes are equally dilatable by heat and equally compreſſible, (gas condenſable) and as theſe two properties depend on one another, as I ſhall prove elſewhere, vapors being equally dilatable with the gaſes, ought alſo to be equally compreſſible: but I muſt obſerve that this latter concluſion cannot be true, unleſs while the compreſſed vapors perfectly retain the elaſtic ſtate; which requires that their temperature be ſufficiently elevated to enable them to reſiſt the preſſure which tends to make them aſſume the liquid ſtate.

That dry and moiſt air dilate alike.—Qu.?

I have aſſerted on the authority of Sauſſure, and my own experiments confirm it, that very dry air, and air holding more or leſs of water in ſolution are equally dilatable; conſequently I am authorized from the whole of the facts to draw the following concluſions.

1ſt, All the gaſes, whatever may be their denſity or the quantity of water they hold in ſolution, and all vapors, equally dilate by the ſame degrees of heat.

Concluſions. Expanſion of gaſes.

2d, The augmentation of volume which each of the permanent gaſes receives, from the temperature of melting ice to that of boiling water, is equal to $\frac{80}{213,33}$ of the primitive volume, if judged by the thermometer divided into 80 parts, and $\frac{100}{266,66}$ of the ſame volume, if by the centigrade thermometer.

Augmentation of volume.

It remains, in order to complete this courſe of experiments, to determine the law of the dilatation of gaſes and vapors, in order to aſcertain the coefficient of dilatation for any degree at a known heat, and to be aſſured of the real progreſs of the thermometer. I ſhall employ myſelf on theſe new reſearches; and when they are terminated, I ſhall have the honour of communicating them to the Inſtitute.

Future experiments on the laws of dilatation at other temperatures.

VII.

New Theory of the Conſtitution of mixed Gaſes elucidated. In a Letter from Mr. J. DALTON.

To Mr. NICHOLSON.

SIR, *Mancheſter, Nov.* 18, 1802.

IN a paper of mine which you had the goodneſs to publiſh in your Journal for 1801 (Quarto Series, V. 241), I announced a new theory of the conſtitution of the atmoſphere. This has ſince been publiſhed on a more enlarged ſcale, and elucidated by a plate, &c. in the Memoirs of the Literary and Philoſophical Society of Mancheſter, Vol. V. Part 2. Notwithſtanding this, I am informed by ſome of my chemical friends, that they do not clearly underſtand the hypotheſis itſelf, and conſequently are not able to judge of its merits or defects: And a late writer (Dr. Thomſon), in his Syſtem of Chemiſtry, Vol. III. Page 270, ſpeaking of the uniform diffuſion of the different gaſes of the atmoſphere, makes the following obſervation: “ Even Mr. Dalton’s ingenious ſuppoſition, that they neither “ attract nor repel each other, would not account for this equal “ diſtribution; for, undoubtedly, on that ſuppoſition, they “ would

New theory of the conſtitution of the atmoſphere, not clearly underſtood.

Farther explanation.

Intended order of diſcuſſion.

“ would arrange themſelves according to their ſpecific gravity.” Now as I am perſuaded that no one acquainted with the principles of mechanical philoſophy would have written the above if he had underſtood my hypotheſis, it ſeems to call from me a further explanation. I propoſe, therefore, *1ſt*, To ſtate in as clear a point of view as the ſubject will admit, the principles which I aſſume: *2d*, To ſhew that the conſequences which I have deduced from them are legitimate, and particularly that mixed elaſtic fluids ought *not* to arrange themſelves according to their ſpecific gravity: and, *3d*, To demonſtrate that the ſuppoſition of the gaſes conſtituting the atmoſphere being held in a ſtate of equal diffuſion by chemical affinity, is not only inconſiſtent with the phenomena, but is completely abſurd.

I. PRINCIPLES ASSUMED.

Principles aſſumed.
1. That particles of ſimple elaſtic fluids repel inverſely as their diſtance.

1. I take for granted that the particles of ſimple (unmixed) elaſtic fluids repel one another with forces inverſely as the diſtance of their centres, the temperature being given. This is a mathematical deduction from the allowed fact, that the ſpace occupied by any gas is inverſely as the compreſſing force: (See Newton's Principia, B. II. Prop. 23). The *abſolute* diſtances of the centres of ſuch particles muſt vary according to circumſtances, and cannot eaſily be determined; their *relative* diſtances in a liquid and aërial ſtate ſometimes may. Mr. Watt has ſhewn, that ſteam of 212° and preſſure 28 inches, is 1800 times lighter than water; conſequently the diſtances of the particles of ſteam are to the diſtances of the ſaid particles in a liquid ſtate, as 12 to 1 nearly, in that particular caſe. Vapor in the vacuum of an air-pump, at common temperature, will have its particles about 4 times the diſtance, or 48 to 1.

2. In mixed elaſtic fluids the heterogeneous particles have no diſtant or gazeſiant repulſion; but only that of contact, formerly called the reſiſtance of impenetrability.
Illuſtration from the viſible effects of magnetiſm.

2. I ſuppoſe that in mixed elaſtic fluids the heterogeneous particles do not repel one another at all at ſuch diſtances as they repel thoſe of their own kind; but that ſuch particles, when brought into actual contact (to uſe the common language), reſiſt each other in all reſpects like inelaſtic bodies. This is the peculiarity of the hypotheſis, and what appears not to be generally underſtood. If I may explain by analogy, the moſt ſtriking will be found in magnetiſm. Two like poles of magnets repel one another with the ſame force whether any

other bodies intervene or not, and do not affect thoſe other bodies; in the ſame way I conceive two particles of any one gas repel one another with the ſame force whether particles of other gaſes intervene or not, and do not affect thoſe other particles. A magnet is amenable to the common laws of motion in its colliſion with other bodies, and when it is brought into ſeeming contact with them; ſo is a particle of one gas when it is brought into ſeeming contact with a particle of another ſpecies: and in this caſe the bodies may be ſaid to have a repulſive power; but this power is eſſentially different from the others, in that it extends to no definite diſtance. Further, conceive a very fine capillary tube placed perpendicular to the horizon, into which let a number of correſpondent ſmall magnetic wires, or particles, be inſerted with their poles of the ſame denomination together, or more ſtrictly, as near as their repulſive power would admit, one particle above another, the air having intercourſe amongſt them. Then, as the magnetic particles would not actually touch one another, by reaſon of their repulſion, they would *ſeem* to be ſupported by the intervening air, whereas in reality they are ſupported and kept at certain diſtances intirely by the repulſion inherent in themſelves, and their own gravitation: and thus I conceive particles of gas ſupport thoſe of their own kind above them, though, were they viſible, they might ſeem to reſt upon others immediately under them; and the ground, or loweſt ſolid or liquid ſurface, by ſupporting the loweſt particle of each kind, has the weight of the whole to ſuſtain. Theſe obſervations, together with a view of the plate above alluded to, muſt, I think, be ſufficient to ſatisfy any one what the hypotheſis is. And it may be proper to add, there is ſomething much reſembling polarity obſervable in the ultimate particles of bodies at the inſtant of tranſition from the liquid to the ſolid ſtate; witneſs the congelation of water.

Particles of gas ſupport each other by repulſion, in the ſame manner; whether the particles of another gas be preſent or not.

2. CONSEQUENCES.

It is plain from the above account, that I conceive any one gas to be conſtituted of perhaps *one* part ſolid matter, and *one thouſand* or more parts vacuity or pore, if it may be ſo called; and that into this vacuity we may throw as many other gaſes as we pleaſe, without materially diſturbing the firſt, provided we do not abſolutely fill the vacuity with ſolid matter, meaning by

If a gas be one part matter and a thouſand mere ſpace, no difference of effect will follow from other gaſes in that vacuity, &c.

And one gas will not raiſe any other, whatever may be their individual ſpecific gravities;

by that expreſſion common liquids or ſolids. Thus we might have had a dozen gaſes in our atmoſphere inſtead of three or four, all in the ſame compaſs, and each retaining the ſame denſity it would have had alone. The heavier gas has no more tendency to raiſe the lighter, than a quantity of ſhot has to expel the air from its interſtices. If therefore Dr. Thomſon, or any other, can ſhew how one fluid, which is not diſplaced nor any way acted on by another, ſhould by its *reaction* cauſe that other to move into a higher or lower ſtation; then the mathematical world will be obliged to reconſider their doctrine of ſtatics. Till then I muſt take the negative of the propoſition, and conclude, that elaſtic fluids of the greateſt and leaſt ſpecific gravity imaginable, on the ſuppoſition I hold, will alike take the loweſt and the higheſt ſtations, regardleſs of each other; or in other words, they will arrange themſelves in the ſame order as if thrown into a complete vacuum. The great difficulty reſpecting the uniform diffuſion of the gaſes being removed, I think on my hypotheſis the other phenomena can require no explanation to any perſon converſant in pneumatics, I will take one inſtance: It may by aſked, How does ſulphuret of potaſh abſtract oxigenous gas out of any mixture; lime water, carbonic acid gas, &c. &c.? The anſwer is obvious: Exactly in the ſame way as if the gas in queſtion was the only one in the veſſel, and the operation going on in a cloſe veſſel.

and the chemical abſorption of any ſingle gas will be the ſame as if it were alone.

3. It is contrary to fact that gaſes are held together by chem. affinity:

3. GASES HELD TOGETHER BY CHEMICAL AFFINITY, ABSURD.

On this head it will be proper to premiſe certain facts:

For they mix uniformly, and remain ſo;

1. When two gaſes of different ſpecific gravity, ſuch as oxigenous and hidrogenous, are put into the ſame veſſel and agitated; if they be ſuffered to ſtand ſome time after, they ſtill continue uniformly mixed.

they occupy the ſame ſpace after mixture;

2. They occupy the ſame ſpace before and after mixture; that is, *one* meaſure of each put together occupy *two* meaſures, the temperature and preſſure remaining the ſame. Mr. Davy ſeems to think this principle is not ſtrictly true in regard to a mixture of azotic and oxigenous gas; but the deviation from it, if any, is extremely ſmall.

and they expand, &c. as before.

3. The compound is ſubject to the ſame laws of rarefaction and condenſation as the ſimples.

There are but *three* ſuppoſitions we can make eſſentially different reſpecting the mutual action of heterogeneous particles of gas: *1ſt*, When two gaſes are mixed their particles may reciprocally *repel* one another, juſt as they act on their own kind in an unmixed ſtate: *2d*, They may be neutral, or have neither attraction nor repulſion for each other: *3d*, They may have a chemical affinity or *attraction* for each other. The advocates for the chemical adheſion of gaſes will agree with me in exploding the firſt, becauſe where nothing but *repulſion* is manifeſt, we can aſcribe no effect to attraction: The ſecond, which is the one I adopt, is obviouſly inconſiſtent with their hypotheſis; and as for the third, I can conceive no other explanation than the following: *1ſt*, Two or more heterogeneous particles may unite and become a new centre for the caloric to adhere to; but in this caſe the gaſes are no longer *two* but *one;* and oxigenous and hidrogenous would become aqueous vapor: This therefore is not a caſe of two *gaſes* being held together by chemical affinity. *2d*, The two gaſes may ſeparately retain their caloric and ſtill be held by chemical affinity; that is, there may be an equilibrium between the powers of attraction and repulſion; but this is evidently inconſiſtent with the third law of condenſation and rarefaction obſerved in ſuch compounds.

Where gaſes mix, the particles muſt either, (1.) repel, or, (2.) have no action, or, (3.) attract.

The gaſes do not *unite* by *repulſion*: if they have no action the author's theory is good; and,

if the particles attract and unite they become *one gas*, and not *two*, as in the aſſumed caſe:

Or if they attract and do not unite, they muſt come nearer together, contrary to the third law or fact above ſtated.

I am, &c.

J. DALTON.

I thank you for the notice you have taken of my Eſſays, and particularly for the note ſubjoined to that on the Expanſion of Gaſes. I have lately read a Paper to the Literary and Philoſophical Society of this place, in which I have ſhewn that the quantity of carbonic acid gas found in a given volume of atmoſpheric air, is not more than $\frac{1}{1000}$ part of the whole; and that the ſaid gas is held in water, not by chemical affinity, but merely by the preſſure of the gas abſtractedly conſidered on the ſurface, forcing it into the pores of the water.

VIII. *A Method*

VIII.

A Method of increaſing the Quantity of Light afforded by Candles, and to obviate the Neceſſity of ſnuffing them. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

SIR,

Feeble light of candles when not frequently ſnuffed.

WHEN our apartments are illuminated with common candles, which cannot be regularly attended with the ſnuffers, they produce much ſmoke, and their glimmering light ſcarcely ſerves the moſt ordinary purpoſes. My firſt attempts to improve this *ſombre* mode of illumination were made many years ago. Thoſe however were not productive of any uſeful diſcovery, but ſome experiments which I made in the year 1797 afforded reſults that rather exceeded my expectation.

Method of remedying this in candles for illuminating apartments.

Experience ſoon convinced me of the utility of the method which I had diſcovered, of uſing candles in ſuch ſituations as above deſcribed, but in every attempt to produce the ſame effect upon my table, I was unſucceſsful, till the beginning of the preſent ſeaſon. This was owing to an overſight, which might be deemed very unaccountable, did not the hiſtory of inventions contain many inſtances of the ſame kind.

Great effects are frequently produced by cauſes that are in general deemed trifles. And, if a trifling alteration be made in the method of uſing common tallow candles, they will become excellent ſubſtitutes for thoſe of wax.

If a candle be inclined it gives a ſteady light, and requires no ſnuffing;

A common candle, weighing $\frac{1}{10}$ of a pound, containing 14 ſingle threads of fine cotton, placed ſo as to form an angle of 30 degrees* with the perpendicular, and lighted, requires no ſnuffing; and what is much more valuable for ſome purpoſes, it gives a light that is nearly uniform in ſtrength without the leaſt ſmoke. Theſe effects are thus produced:

becauſe the wick protrudes into the air, and becomes burned, &c.

When a candle burns in an inclined poſition, moſt part of the flame riſes perpendicularly from the upper ſide of the wick, and when viewed in a certain direction, it appears in the form of an obtuſe angled triangle. And as the end of the wick projects beyond the flame at the obtuſe angle, it meets with the air, and is completely burnt to aſhes; hence it is rendered incapable of acting as a conductor to carry off any part of the combuſtible matter in the form of ſmoke. By this ſpontaneous mode of ſnuffing, that part of the wick which is acted upon by the flame, continues of the ſame length, and the flame itſelf very nearly of the ſame ſtrength and magnitude *.

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* Candleſticks may be made to hold the candle at this angle, or they may be ſo contrived as to hold the candle at any angle at pleaſure. I have candleſticks conſtructed on both principles.

The light does not undulate.

The advantages which may be derived from candles that require no ſnuffing and afford no ſmoke, may be readily underſtood; but theſe candles have another property which ought not to be paſſed over in ſilence. A candle ſnuffed by an inſtrument gives a very fluctuating light, which, in viewing near objects, is highly injurious to the eye; and this is an inconvenience which no ſhade can remove. But when a candle is ſnuffed ſpontaneouſly, it gives a light ſo perfectly ſteady and ſo uniformly bright, that the adjuſtments of the eye remain at reſt, and diſtinct viſion is performed without pain, and without uneaſineſs.

Candles which were ſubjected to experiment.

Candles on which I have made experiments, are deſcribed in the following

TABLE.

No.	No. of candles to the pound avoirdupois weight.	Length in inches.	No. of ſingle threads of fine cotton in the wick.
1	14	8.5	10
2	12	9.	12
3	10	9.75	14
4	8	10.	20
5	6	10.25	24
Mould	6	13.	

Numbers 1, 2, and 3. Theſe candles, when lighted and placed to form an angle of 30° with the perpendicular, require

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* The wick's not being uniformly twiſted throughout, may occaſion a little variation in the dimenſions of the flame.

no ſnuffing: they give lights which are nearly equal, and combuſtion proceeds ſo regularly, that no part of the melted tallow eſcapes unconſumed, except from accidental cauſes.

Remarks on the phenomena.

No. 4, placed at the angle mentioned above, and lighted, requires no ſnuffing: it gives a light very little ſtronger than No. 1, but its colour is not quite ſo white, nor its flame ſo ſteady.

No. 5. This candle, placed at an angle of 30°, and lighted, requires no ſnuffing; its flame is rather fluctuating, and not ſo white as No. 4, nor is its ſtrength of light much greater than No. 1. The melted tallow ſometimes overflows when the air in the room is put in motion; yet the light of this candle is much improved by the candle's being placed in an inclined poſition.

The mould candle, treated in the ſame manner as thoſe above, affords a very pure ſteady flame, without ſmoke and without ſnuffing, and its ſtrength of light is about equal to that of No. 1.

ON ECONOMY.

The economy of candles as to light and conſumption of the fuel.

My experiments have not been ſufficiently numerous to determine with preciſion which of theſe candles affords the moſt light at a given expence, but the few experiments which I have made ſeem to indicate, that the quantity of light is directly as the quantity of combuſtible matter expended.

EZ. WALKER.

Lynn, Nov. 19, 1802.

REMARKS.—W. N.

On candles in general.

On candles in general ſee an eſſay by W. N. in the Philoſ. Journal, quarto, I. 67; alſo my Firſt Principles of Chemiſtry, Art. *Wax:* Hermſtadt on the different materials, wax, tallow, ſpermaceti; Philoſ. Journ. quarto, V. 187.—I have always ſuſpected from the general facts, that the light of ſmall candles is cheaper than that of larger, and have little doubt of the truth with regard to ſuch as exceed the diameter of three quarters of an inch in tallow. We are much in want of a ſeries of experiments on the different deſcriptions of candles, under the general

Outline of very deſirable experiments concerning them.

general titles of Wax, Spermaceti, Tallow, and mixed; and tabulated, to ſhew, 1. Average weight of the wick; 2. Number of threads; 3. Weight of the whole candle; 4. Diameter; 5. Length; 6. Time of combuſtion per inch; 7. Ditto per ounce; 8. Quantity or intenſity of light when newly ſnuffed; 9. Ditto after burning half a minute, or till the uſual period of ſnuffing; 10. Average intenſity; 11. Expence of material per hour to produce the quantity 1.00 of light; 12. Ditto in the money price of the article; 13. Station of the barometer; 14. Ditto of the thermometer; 15. And ditto of the eudiometer. I am abſolutely unacquainted with any good ſtandard of compariſon of light. The ſun perpetually varies with the weather, its altitude, &c. It ſeems probable that an earthen lamp, having a wick of known texture and weight, cut to a determinate elevation by a gage, and charged with very pure olive oil, might, at like ſtations of the barometer and thermometer, give light ſo nearly equal, for the firſt two or three hours, as to afford reſults conſiderably exact; of which the means of ſeveral repetitions would be of great practical utility. The compariſon of lights by the ſhadows has been often mentioned; but the ſimplicity and value of the method (not yet univerſally known) will juſtify a ſhort repetition. When the ſhadows of the ſame object fall upon each other, not preciſely, the bordering ſhades will be ſeverally illuminated by one light only: move the lights till theſe bodies appear equally dark at the point or place where they meet in an angle. Then meaſure the diſtances of the lights from that point or place, and the intenſities are inverſely as the ſquares of thoſe diſtances. It is to be noticed that the obſtacle which forms the ſhadow, a book for example, muſt be held ſo that the ray from each light may fall on the wall nearly in the ſame angle; and this will always be the caſe if the farther light be nearly behind the other.

Standard of the intenſity of ligh unknown.

Method of comparing lights by the ſhadows.

In a ſubſequent letter, Mr. Walker informs me, that he carefully meaſured his intenſities by the method of ſhadows, as mentioned in Prieſtley's Optics. I conclude this to have been the method here deſcribed; though, for want of a good index, I can only find the method of Bouguer in that work.

IX.

Deſcription of a new Secret Lock. *By* J. B. BERARD.

(Concluded from Page 222.)

onſtruction of ſecret lock ac- eſſible only to he proprietor.

I MAY alſo in this place ſtate the reaſon why I have made the key as well as the ferrils with 24 rather than 25 diviſions. In the firſt place, it affords a ſmall diminution in the dimenſions of the lock. Another more eſſential advantage is, that in the diviſion of 24, each notch in the ferril is oppoſed to another diametrically oppoſite, which facilitates the introduction and taking out of the bolt. We may obſerve, that for the ſame reaſon the teeth of the ferrils, being rather rounded at the tip, facilitate the entry of the little wedge, which is likewiſe rounded underneath.

The lock being thus conſtructed, it only remains to fix it to the door. For this purpoſe, an opening is contrived of the ſize of the box of the lock, in the horizontal croſs piece, without cutting the vertical frame of the door; and round it a groove is made, of 2 in depth and 6 in breadth, on which the face of the lock reſts: the lock is placed in this opening without the bolt; and if it is deſired, four nails or ſcrews may be driven into the wood through four holes made in the face of the plate; but this is not neceſſary. The bolt is then placed on the other ſide of the door, and if the wood be too thick, it may be cut away till the bolt ſlides fair and eaſily over the wood. This is very eaſy to be done, and then the face of the bolt ought to lie fluſh with the edge of the door.

It is obvious that the height of the ferrils muſt be proportioned to the thickneſs of the door. If the door be 30 m. thick, for example, the ferrils (which ought to be 1 under the ſurface of the lock) would be about 29.

I muſt alſo obſerve, that in order to render the motion of the ſcrew-heads more eaſy and uniform, a paſteboard of 1 in thickneſs, with five holes punched in it, muſt be laid upon the inner ſurface of the plate of the lock; and above the paſteboard, another ſlip of very thin braſs, likewiſe perforated with five holes: then the interior flat wheels have a very eaſy motion

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over the braſs which reſts on an elaſtic plane, which yields to the unavoidable irregularities of execution in a work of a moderate price.

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It muſt likewiſe be obſerved, that the heads of the ſcrews, as well as the face itſelf of the lock, ought to be caſe hardened, in order to protect it from external injuries, and to prevent the enlargement of the holes which receive the ſcrews. The ſurface ought to be highly poliſhed, and the heads of the ſcrews ſhould be blue.

Advantages of the new Lock.

1ſt, As in Reignier's lock, which likewiſe has ſcrew-heads, ferrils, and teeth without wedges, theſe pieces cannot be put at diſtances extremely preciſe, it neceſſarily follows that one of the pins or teeth, the fourth for example, will always meet its ferril before the others meet theirs. Let us ſuppoſe that with a turnſcrew we endeavour to turn the ſcrew-head which moves the bolt, whilſt with another hand and turnſcrew we ſucceſſively turn the other ſcrew-heads, they will all turn freely, except a fourth, which will be immoveable: It will be concluded that the fourth ſcrew-head is not in the poſition to open the door, that is to ſay, that the deep cut of the ferril is not oppoſite the pin of the bolt: that figure or letter of the ſcrew-head will be noticed among thoſe which do not form the ſecret. The poſition of the ſcrew-head muſt therefore be changed, and the above proceſs repeated till the pin is felt to fall into the notch. The ſame experiment muſt then be made with that ſcrew-head among the remaining ferrils which alone is now preſſed by its pin. And after eighty attempts at moſt, if there be eight ſcrew-heads, each having eleven figures, we ſhall know the eight figures which compoſe the ſecret.

Here then is a regular and infallible proceſs to diſcover in leſs than an hour the combinations with which the lock is cloſed. This defect, which is common to the generality of compounded locks, does not exiſt in mine.

When my bolt is moved, the wedges which precede the teeth enter at the ſame time into the cuts of the ferrils, which are of ſuch a height that they graze the interior ſurface of the lock: all the four ferrils then become immoveable, and it is impoſſible to diſcover which is the ferril that is firſt met by the correſpondent tooth, becauſe their fixation is produced at the

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ſame time from the teeth and the wedges. No external examination can therefore ſhew the difference in ſituation of the four ferrils. It is obvious that in order to inſure this effect, the wedges ought to fit the cuts between the teeth in the ferrils exactly; which is very eaſily obtained in the conſtruction. Another advantage reſulting from the wedge is, that if any of the deep notches or openings be not exactly in a line with the reſt, the wedge eaſily adjuſts it, and the bolt is not in the leaſt impeded on that account; this advantage renders it unneceſſary that the workmanſhip of the lock ſhould be exquiſite, a condition which, in a work of a moderate price like the preſent, would render it uſeleſs.

2d, My lock is eaſily opened, it having only four ſcrew-heads: it has nevertheleſs 331,776 * combinations, a number fully ſufficient to ſet the moſt ſuſpicious miſer at eaſe.

We may add, that it is much eaſier to recollect one word than a number, which muſt be the caſe in Reignier's lock.

3d, When characters are engraven externally on the ſcrew-heads, they are either difficult to be perceived, or may be eaſily damaged. A key with characters engraved on it, is I think a great improvement: a ſaving is likewiſe afforded, which is a conſideration of ſome conſequence in a machine of common uſe.

4th, The method by which I vary the poſition of each ferril with reſpect to its ſcrew-head, is at once ſimple and ſolid.

5th, The mechaniſm by which the bolt is attached to the lock, is concealed, and cannot be damaged. As nothing is ſeen of the lock but a ſmooth plate on either ſide, it would be very proper for a bureau or 'ſcrutoire.

Another ſingular property reſulting from this mechaniſm is, that it can only be taken to pieces by the proprietor, whether the door be open or ſhut: it could not even be done by a perſon ſhut in the apartment.

6th, The lock may eaſily be ſet: as the interior ſcrew-heads do not turn on wood, there is no danger that their motion ſhould become leſs regular or eaſy on account of wear. The ſame may be obſerved of the bolt, which ſlides in a groove of iron.

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* This is the fourth power of 24.

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7th, The lock is very ſolid; for there is no opening into which any tool might be introduced. The face of the door may likewiſe be ſurrounded with a ſtrong metallic covering let into the wood, and the back part of the door may be covered with iron plates, the only method of preventing any one from cutting the wood round the lock.

8th, If it ſhould be thought inconvenient to place the lock in the thickneſs of the door it may be eaſily avoided. The ſtems of the ſcrew-heads need only be made longer, and another plate ſimilar to that of the face, added: the ſcrews in that caſe, after having paſſed through the face as well as the door, would paſs into the lock. This addition requires an increaſe of work, but it affords no real advantage.

Lock for a Bureau or Secretary.

Though my lock, as I have deſcribed it, can be eaſily fixed to a ſecretary, yet I think it uſeful to indicate the changes convenient to be made in it.

1ſt, The height of the ferrils muſt be diminiſhed, that the bolt may be intirely ſunk in the flap of the ſecretary.

2d, In order to diminiſh the length and breadth of the lock, ten diviſions in the ferrils may be made inſtead of eighty: they need then be only 25 in diameter; but then the ſecret will conſiſt in a number of four figures inſtead of a word of four letters, which is not ſo convenient, as I have already obſerved.

3d, The box of the lock may be ſuppreſſed, and the lock will ſtill have ſolidity ſufficient for a bureau; the price will likewiſe be diminiſhed.

4th, Inſtead of engraving characters on the face of the lock, I prefer, as has been ſeen, to mark them on the turnſcrew itſelf; but the external damage is not to be feared in a ſecretary as in a door. It is therefore more convenient that the figures or letters ſhould be on the face itſelf of the ſcrew, and a knob or ſmall handle may be contrived on the face of the ſcrews, in order to turn them. In this caſe no key or turnſcrew need be carried about; this is an advantage of ſome conſequence.

Concerning ſome other Kinds of Compound Locks.

It is poſſible, as I have already obſerved, to apply the principles of theſe combinations to the conſtruction of locks in a

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number of methods. That which I have deſcribed is the laſt, reſult of my endeavours on this object; it ſeems to me to be the ſyſtem that beſt anſwers all the conditions of the problem. I think, however, that it will not be uſeleſs to make known ſome more, if it were only to prevent others from making unneceſſary attempts on the ſame ſubject.

1ſt, The front part of the lock preſents 25 ſmall rectangles, 10 wide by 15, diſpoſed in a right line, and forming in all 250. The 24 letters of the alphabet are marked on theſe rectangles: they are included between the two partitions of the horizontal channel or groove, and they all touch one another. They may be pulled outwards to the diſtance of 4 millemetres, like the ſtops of an organ. Each of theſe ſtops has a little ſpring at top, which gives it an eaſy and ſmooth motion, and prevents it from dragging with it any of its neighbours. Each ſtop, in the inſide of the lock, has a projection in the form of a tooth.

The bolt has, on its ſuperior and horizontal face, 24 dovetailed grooves with dovetails, each of which receives a regiſter or ſtop of the ſame form, having a tooth at top. Theſe ſtops of the bolt may be put into their grooves with the extremity having the tooth, or the other end, juſt at pleaſure. The bolt is encloſed in a box, the viſible and interior face of which moves in a groove, in order to take out and replace the bolt at pleaſure.

The lock is thus uſed. If it is to be opened, the exterior ſtops forming the ſecret are brought forward, then the 25th ſtop is drawn, and this renders the reſt of the ſtops immoveable. After having returned it, it is turned by means of a knob or handle which it carries, and the bolt is thus made to move.

When the ſecret is to be changed, the interior regiſters are to be placed in the grooves cut in the bolt, by the extremity which has no tooth. They are then inverted with reſpect to the others, and their teeth become in a line with thoſe of the exterior ſtops.

The number of combinations in this lock is equal to the eightieth power of 2, nearly 17 million: It is convenient for a bureau, but the workmanſhip muſt be exquiſite: Beſides, it is not convenient for a door, becauſe ſtops which project ſeveral millimetres may be eaſily damaged. I cannot here enlarge upon the other imperfections which it preſents. In this ſyſtem the

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the 24 ſmall ſcrew-heads might be ſubſtituted for the ſtops, which would not then require ferrils. This arrangement would not be preferable to the other.

2d, Let us imagine three rings lodged one within the other, and forming one and the ſame plane with a fourth fixed to the door, and an immoveable circle in the centre; ſuch is the face of the lock. The three intermediate rings turn freely and ſeparately by means of a button, which muſt be made to correſpond with one of the letters of the alphabet marked on the outer circle fixed to the door. Each carries interiorly a ferril capable of 24 poſitions. When the notches of the three ferrils are diſpoſed in ſtraight lines, the bolt is made to move by means of a knob or handle in the middle of the face.

This lock* reſembles in ſome reſpects the firſt I have deſcribed. In this the ferrils are concentric, and in the others they are ſeparate. It has an agreeable form, and would be proper for a ſecretary, and its execution does not require much preciſion. It is not very convenient for a door, becauſe it is likely to be damaged.

3d, Let us imagine five ſcrew-heads on the face; let each of theſe ſcrews carry a ſmall pinion within, which moves a notched bar or rack. This rack muſt have ten teeth, one of which is withdrawn, in order to leave a clear way or ſpace. The fifth ſcrew muſt move a fifth rule carrying four teeth, and at right angles to the other four. It is clear that this laſt rule, which I ſuppoſe to be the bolt, can never move except when the other four rules are in a convenient poſition. In this method the lock will be enormouſly large; and it will alſo be very difficult to prevent this ſecret from being diſcovered from without.

4th, If toothed wheels, with an unequal number of teeth all taking in one another, be ſubſtituted for the flat wheels which are under the ferrils in the ſyſtem of locks with ſcrew-heads, then, if one ſcrew-head be moved, it will derange all the ferrils at once. This ſaves time, without diminiſhing the number of combinations. This advantage led me to approve of the conſtruction for ſome time; but it is productive of ſo many ſerious inconveniences, that I was obliged to abandon it.

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* I have ſeen ſome at Paris, nearly ſimilar to this.

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It would be ſuperfluous to examine a greater number of ſyſtems. I ſhall conclude by one general obſervation.

All the compound locks that can poſſibly be made, may be divided into three ſpecies. In the firſt, ſtraight or curve lines are made to move; in the ſecond, plain or curved ſurfaces; in the third, ſolids. It is obvious that one ſolid offers as many arrangements or combinations as three lines together, and one ſurface as many as two lines.

Theſe lines, ſurfaces, and ſolids, form interior ſtops made to receive motions (externally produced) in a ſtraight or in a curve line.

It is obvious from the application of theſe principles, an almoſt infinite variety of locks may be conſtructed. I ſhall not undertake to extend the variations, but ſhall only ſay, that I think I am convinced that the beſt ſyſtem is that of moving the circumferences of circles internally by circular motions produced from without.

CONCLUSION.

I am ſenſible that what I have ſaid concerning the locks, which I conſider as leſs perfect, is not ſufficient to direct their conſtruction; but I have ſaid enough to make their ſyſtem known to artiſts, and to deter them from fruitleſs experiments. It muſt not be forgotten, that in this kind of machine nothing good or uſeful ſhall be done, unleſs ſimplicity, convenience, ſolidity, economy, and facility of execution, be combined.

It is under theſe ſeveral points of view that the problem of compound locks, which may be worthy of the attention of artiſts, ought to be conſidered. I do not pretend to have diſcovered the beſt, but I dare aſſert, that the lock which I have deſcribed approaches ſo near to perfection, as to entitle it to the ſuperiority over the common locks which are more or leſs ingenious and complicated, and even to place it among the locks of combination, as one of the moſt ſimple that has yet appeared, or that I know of.

Whether I ſhall be allowed the ſmall honour of having invented this arrangement, or the merit only of having brought it to perfection, is not to me a matter of conſequence. I ſhall be ſatisfied if I have aſſiſted others in improving this ſpecies of machines, the utility of which need not be proved. In fact, if the common locks which are brought to the greateſt perfection,

tion, were rendered as ſure and inviolable, as they are at preſent the contrary, they would ſtill have this inconvenience: they are liable to be forced by introducing ſome tool or inſtrument; and even if the violator ſhould fail in his attempt, the proprietor will nevertheleſs be reduced to the neceſſity of breaking open his own door if the inſide of the lock ſhould be ever ſo little deranged. The advantage then of a lock which affords no entrance to a key is obvious, a problem which I think cannot be well ſolved, except by a circular motion. That which I have deſcribed, not only anſwers all the conditions of the theory, but has ſtood the teſt of experience. The greateſt obſtacle to the propagation of locks of combination, proceeds from the prejudices of habit, ſtill more than from their want of ſimplicity: and it is well known that this is the fate of all truths *.

X.

Deſcription of Dr. YOUNG'S *Apparatus for exhibiting the Colours of thin Plates, by means of the Solar Microſcope. From the Journals of the Royal Inſtitution,* I. 241.

The colours of thin plates have been little examined, and never publicly exhibited.

THE colours of thin plates were obſerved by Boyle and Hooke, and more accurately analyſed by Newton: but little or nothing was added to the account that Newton gave of them, until ſome attempts were lately made to explain them, and to build at the ſame time on the explanation, the principal arguments in favour of a new ſyſtem of light and colours. The phenomena themſelves were very little known, except from Newton's deſcription; it had happened but to few to obſerve them: and they had never been made conſpicuous to a public audience in a form equally beautiful and intereſting.

New attempt to exhibit them by the ſolar microſcope.

It appeared, however, that there would be little difficulty in applying the apparatus for repreſenting opaque objects in the ſolar microſcope, to the exhibition of theſe colours on a large ſcale: but ſeveral precautions were neceſſary, in order to obtain the moſt advantageous repreſentation; and, theſe precautions having been completely ſucceſsful, it may be of ſome utility to give a detached account of them.

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* In the former Paper for Requier read Reignier, and in the note on p. 217, for Braviali read Bramah.

Methods of producing the colours.

The colours of thin ſubſtances muſt often have been ſeen in bubbles of water or of other fluids, and in the film produced by a drop of oil ſpreading on water; they were more particularly obſerved in the plates of talc, or of ſelenite, into which thoſe ſubſtances readily divide. Sir Iſaac Newton made his experiments principally on the colours of ſoap bubbles, and on thoſe which are produced by the contact of two lenſes. For inſpecting the colours of ſoapy water, the moſt convenient method is that of Mr. Jordan. He dips a wine glaſs into a weak ſolution of ſoap, and then holds it in a horizontal poſition againſt an upright ſubſtance, for example, a window ſhutter; the film covering the glaſs being in a vertical poſition, the gravity of the fluid tends to make it thicker at the lower part, and it becomes every where gradually thinner and thinner, till at length it burſts at the uppermoſt point. The colours aſſume, in this caſe, the form of horizontal ſtripes, ſimilar to the rings which are to be more particularly deſcribed.

Mr. Jordan's convenient method of exhibiting the ſoapy film.

The colours from a denſe plate are more vivid than thoſe from a rare one; becauſe the latter are diluted by the light reflected from the confining mediums.

It has been obſerved by Newton, that the colours thus reflected from a plate of a denſer medium, are more vivid than when a plate of a rarer medium is interpoſed between two denſer mediums. But the cauſe of this apparent difference is probably, the quantity of foreign light that is generally preſent in the experiment, reflected as well from the upper ſurface of the ſuperior medium as from the lower ſurface of the inferior, both theſe ſurfaces being often nearly parallel to the ſurfaces in contact. It becomes therefore deſirable to remove this foreign light: this may be done effectually, by employing one glaſs in the form of a priſm, and coating the lower ſurface of the other with black ſealing wax: the light reflected by the oblique ſurface of the firſt, is thus thrown into another direction; and the reflection of the inferior ſurface of the ſecond, is either deſtroyed or rendered imperceptible. And, with theſe precautions, the rings of colours produced in the reflected light, may be rendered a very beautiful object by means of the ſolar microſcope.

Remedy.

The plane glaſs is made priſmatic, and the lower ſurface of the lens coated black.

The moſt perfectly plane glaſſes are thoſe which are uſed for Hadley's quadrants: one of theſe may be ground in the direction of the diagonal of its tranſverſe ſection, ſo as to make a thin wedge or priſm; and the ſurface of the lens employed muſt be a portion of a ſphere of from five to ten feet radius.

The two glaſſes muſt be retained in their poſition by means of three ſcrews; for, as ſoon as the preſſure is removed, they repel each other with conſiderable force; and, for this reaſon, neither of them ought to be very thin, otherwiſe they will bend before they are ſufficiently near.

This apparatus may be fixed in the ſolar microſcope.

For adjuſting the glaſſes to the microſcope, it is convenient to fix them in a cylinder of ſufficient ſize to project beyond the glaſſes and their ſcrews, in order that they may be readily turned ſo as to reflect the light coming from the ſpeculum into the direction of the axis of the microſcope: it is obvious that, in this caſe, they muſt be ſomewhat inclined to the light, ſo that the focus of the whole image will never be equally perfect; and, inſtead of being circular, like the rings themſelves, their images on the ſcreen will be oval. In this manner, eight or ten alternations of colours may eaſily be obſerved; but their order and ſequence is too complicated to be eaſily underſtood: for they are really compoſed of an infinite number of ſeries of rings of different magnitude, each ſeries being formed by each of the gradations of light in the priſmatic ſpectrum, which, near the centre, are ſufficiently ſeparate to form diſtinct appearances, either alone or in combination; but, after eight or ten alternations, are loſt in the common effect of white light. For, when the glaſſes are illuminated by homogeneous light only, ſeparated from the reſt by the refraction of a priſm, or otherwiſe, the rings of each colour occupy, together with the dark ſpaces, the whole viſible ſurface, their number being only limited by the power of the eye in perceiving objects ſo minute as the external ones become, in conſequence of the rapid increaſe of the thickneſs of the plate of air near the edges of the curved ſurface. This circumſtance being once underſtood, it is alſo capable of being illuſtrated in a manner ſtill more elegant, by placing a priſm a few feet from the microſcope, leaving only a narrow line of its ſurface expoſed to the incident rays, and then throwing the rings of colours on it, in ſuch a direction that this line ſhall paſs through their centre. Care being taken to exclude from the priſmatic ſpectrum thus formed all extraneous light, it exhibits a moſt intereſting analyſis of theſe colours; for the line conſiſts of portions of the rings of all poſſible gradations of colour, each forming a broken line, but not of the ſame dimenſions; and, by the priſmatic refraction, all theſe broken lines are ſeparated and placed parallel

The effect

with homogeneous light,

and ſtill more elegantly by a priſm.

Intereſting effect.

rallel to each other, on account of the different refrangibility of the light of which they conſiſt. Thus the broken line of the extreme red, which conſiſts of the longeſt portions, is leaſt refracted: the other reds follow, and are placed in contact with the firſt, and with each other, but on account of the different magnitude of the portions, ſomewhat obliquely. The dark ſpaces alſo are in contact, and form a ſeparation between each portion of light. In the ſame manner, the green follows the red, with little or no viſible yellow. The blue and violet are ſomewhat mixed: for theſe two colours are much leſs widely ſeparated by thin plates than by the priſm: for this reaſon, each portion of light formed by the contiguous lines of the different colours is bounded not by ſtraight but by curved lines.

It is evident that, by drawing a line acroſs this compound ſpectrum at any part, we may learn the component parts of the light conſtituting the rings at that part; for the priſm only ſpreads the colours in a direction tranſverſe to this ſpectrum: and it may be obſerved, that after the eighth or tenth alternation, the light tranſmitted at each point is ſo mixed, that we may eaſily underſtand how it appears white.

Colours by tranſmiſſion are not ſo advantageouſly ſhewn.

The colours of thin plates, as ſeen by tranſmiſſion, are alſo eaſily exhibited in the ſolar microſcope; but, ſince it is utterly impoſſible to exclude the very great proportion of the light which is not concerned in their formation, they never appear ſo brilliant as the colours ſeen by reflection.

Y.

XI.

Extract of a Letter from R. CHENEVIX, *Eſq. on the Magnetic Property of Nickel.*

Nickel is magnetic.

I AM afraid that I was too haſty in denying the magnetic property of nickel. The fact is, I had obtained both it and cobalt in that ſtate. But in proſecuting a ſeries of experiments upon theſe metals, I perceived the real cauſe why ſeveral fragments which I had obtained did not exert any influence upon the magnet. I took a piece of nickel which was not attractable, and which exhaled arſenical vapours when heated by the blow pipe, and diſſolved it in nitric acid. I boiled the ſolution until the whole of the metal was converted into arſeniate of nickel; I then dropped in a ſufficient quantity of nitrate of lead, and evaporated the liquor at a very gentle heat, but not quite to dryneſs. Alcohol poured into this ſolution precipitated every ſalt but the nitrate of nickel, which had been formed by the double decompoſition of the arſeniate of nickel and the nitrate of lead. This is the method which I uſed in the analyſis of the arſeniated copper ores of Cornwall; and the proportion of arſenic acid, contained in arſeniate of lead, was the ſtandard to which I referred thoſe ores to aſcertain their relative proportions. Theſe experiments were publiſhed in the Philoſophical Tranſactions for 1801.

Examination of a piece not magnetical.—Solution in nitric acid; ebullition; nitrate of lead added; evapor. nearly to dryneſs; alcohol added which diſſolved nitrate of nickel only;

After the preceding operations, I evaporated the alcohol of the ſolution of nitrate of nickel, rediſſolved the metallic ſalt in water, and decompoſed it by potaſh. The oxide, well waſhed and dried, was reduced in a Heſſian crucible, lined with lamp black; and in that ſtate it manifeſted ſtrong magnetic properties. None of the proceſſes of the reagents I had uſed could have communicated iron to the nickel. I then allayed this ſame nickel with a ſmall proportion of arſenic in a ſimilar crucible, and found that it had loſt its magnetic property. A ſmall portion of iron added to the maſs did not render it attractable. From theſe experiments, often repeated, I believe that arſenic has the power of concealing the magnetic property of nickel. Cobalt appears to be in the ſame caſe. I was therefore miſtaken when I announced, that thoſe metals were not in themſelves magnetic.

evap. of the alcohol; ſolution in water; precip. by potaſh; reduction *with charcoal? It was ſtrongly magnetic.*

Arſenic being added deſtroyed the magnetic property.

A little iron did not revive it.

XII.

On the unexpected Production of Sulphate of Magneſia. By J. BOSTOCK, *M. D.*

To Mr. NICHOLSON.

SIR,

Liverpool, Nov. 18, 1802.

I FEEL ſomewhat doubtful whether the ſubject of the following communication may appear to you ſufficiently intereſting, or the experiments detailed with a ſufficient degree of accuracy, to entitle it to a place in your Journal. I have however ſent it for your inſpection; if you think it of any value, it is completely at your ſervice.

I remain,

your's, &c.

JOHN BOSTOCK.

Copious ſaline efflorescence on the inner ſurface of a brick wall.

ABOUT two years ago, a houſe was erected in the neighbourhood of this town by Tho. Earle, Eſq. upon his eſtate of Brook Farm. The walls, which were chiefly conſtructed of ſtone, were lined with bricks, formed of clay procured from the foundation. Some months after the erection of the building, a large part of the bricks were covered with an extremely copious, ſaline effloreſcence. Mr. Earle felt anxious leſt this peculiar ſubſtance ſhould prove injurious to the plaiſter, with which it was propoſed to cover the walls, and was deſirous to be informed reſpecting its nature. With this intention the following experiments were performed.

Analytic experiments to determine its nature.

The ſalt was diſſolved in water, filtered, and gently evaporated; it cryſtallized with difficulty, and the cryſtals were too indeterminate to aſcertain preciſely their figure. The ſalt thus purified had a bitter, acrid taſte; it neither effloreſced nor deliqueſced after being expoſed for ſeveral months to the atmoſphere. It was very ſoluble in water; 100 grains of water at the temperature of 55°, diſſolved nearly 50 grains of the ſalt; and the ſame weight of water at the boiling heat, diſſolved nearly three times that quantity.

1. To a dram of the aqueous ſolution, 20 quarts of ſolution of the muriate of barites were added, and a copious, denſe, white precipitate was inſtantly produced.

2. Equal parts of the aqueous ſolution and lime-water, when added together, formed a cloud which gradually ſubſided.

3. Twenty drops of pure potaſh were added to a dram of the ſolution, and a copious precipitate was inſtantly produced.

4. To a dram of the ſolution were added two drams of a ſolution of the carbonate of potaſh, a flocculent precipitate was immediately produced.

5. A very copious precipitate was produced, when twenty drops of pure ammonia were added to a dram of the ſolution.

6. The oxalic acid produced no action when added to the ſolution.

7. Ten drops of the nitrate of ſilver being added to the aqueous ſolution, produced ſcarcely any effect; a ſlight milkineſs only was perceptible in the fluid.

It was ſulphate of magneſia.

Remarks.

From theſe experiments the nature of the ſalt is ſufficiently evident; the action of the muriate of barites proves it to be a ſulphate, and the experiments with lime water and the alcalis, ſhew that the ſulphuric acid is united to magneſia. From the 6th experiment we find that it contains no lime, and from the 7th, that it contains only a very minute portion of muriatic acid. It appears therefore to be the ſulphate of magneſia, or Epſom ſalt, in almoſt a perfectly pure ſtate. The ſulphate of magneſia, which is uſually met with in commerce, contains a conſiderable proportion of muriatic acid; this is proved by the very copious precipitate which it will be found to yield upon the addition of the nitrate of ſilver. To the muriatic acid which it contains, Bergmann aſcribes the tendency which the common ſulphate of magneſia poſſeſſes, of attracting moiſture from the atmoſphere.

The reſult of theſe experiments were not what I expected; I imagined that the efflorescences which we occaſionally obſerved on walls, were compoſed for the moſt part of the nitrate of potaſh. The purity of the ſalt was alſo a circumſtance which appeared curious, as being contrary to the ſtate in which we uſually find it. The moſt ſingular circumſtance, however, was its appearing at all in that ſituation.

The ſalt was not found in the clay of the bricks.

Upon an examination both of the water and the clay, with which the bricks were made, there are but faint indications of the exiſtence of this ſalt, at the ſame time that the preſence of the muriatic acid is very perceptible. Is it poſſible that the muriatic acid may have been diſſipated during the burning of the bricks, and the ſulphuric generated by the union of oxigen with ſome ſulphureous matter exiſting in the coals? I fear this conjecture will be thought extravagant, but at preſent I ſee none which is more probable.

Conjecture.

This incident did not injure the plaiſter.

It is neceſſary to add, that no injury to the plaiſter appears to have ariſen from the preſence of this ſulphate of magneſia.

XIII.

Abſtract of a Memoir on the Dew. By Cit. Benedict Prevost *.

Well known fact that dew falls more plentifully, or in preference, on glaſs and not on metals.

It is known that a glaſs veſſel is ſometimes covered with dew in the midſt of a veſſel of ſilver, which at the ſame time remains dry; and that mercury in a china veſſel is not lowered by the dew, though the edges of the veſſel are very wet; and it is very generally concluded from various experiments of this kind, that glaſs is a ſubſtance upon which the dew is moſt plentifully depoſited, while it is not at all precipitated on metals.

Repetition of the experiments.

Cit. Prevoſt was curious to repeat theſe experiments, which did not ſeem to him to have been made with ſufficient care, in order to determine to what degree of preciſion it might be allowable to ſupport the uſual concluſion. In this repetition he obſerves ſome very ſingular facts, of which we ſhall proceed to give the general reſults.

Diſcs of metal upon glaſs were often changed with dew; tho' more frequently they are dry and the glaſs wet.

1. Diſcs of tinfoil, of gold, of ſilver, of copper, &c. being applied or ſtuck to plates of glaſs; and expoſed to the dew, were often found to be no leſs charged with dew than the glaſs itſelf on which they were placed; though it more commonly happens that they remained dry, while the glaſs was very wet.

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* Read to the Society of Sciences and Arts of the Department of Lot, ſitting at Montauban. Tranſlated from the Annales de Chimie, XLIV. 75.

2. In this caſe the preſerving property of the metal frequently extends to a conſiderable diſtance round, ſo that in the midſt of the humidity, there is conſtituted a zone perfectly dry and almoſt always well terminated. But what is very remarkable is, that when the glaſs is moiſtened underneath, the place anſwering to the metal remains dry; ſo that the influence of the metal acts through the glaſs of ſeveral millimetres (or twentieths of an inch) in thickneſs. And we ſhall ſoon ſee that it extends much farther.

The metal is not only dry, but cauſes a dry place round it.

and even on the oppoſite ſurface of the glaſs.

3. The metal does not in this caſe act as a ſhelter or defence, for if the ſurface be covered with glaſs nothing particular happens.

An additional glaſs plate does not produce the effect of the metal.

4. Having paſted (*collé*) againſt the upper part of a pane of glaſs in a window expoſed to the north, and on the inner ſurface a metallic rectangle; and on the lower part of the ſame pane externally a ſimilar rectangle, the author obſerved, that when the moiſture was condenſed on the inner ſurface, as often happens in winter, the inner metal ſo far from remaining dry, was more moiſtened than the glaſs, while the ſurface correſponding with the external metal was perfectly dry, as well as a ſquare of ſeveral lines round about. It was however remarked, that the humidity in the ſquare approached much nearer the corners of the metal than in the middle of the ſides of the rectangle.

A metallic plate being paſted on the pane of a window within; and another on a different part without; moiſture condenſed within fell moſt on the inner metal, and none on the ſurface correſponding to the outer;

3. In the ſame arrangement if the humidity was depoſited on the outſide (by cooling) the external metal remained dry, and the place correſponding with the internal metal was on the contrary rather wetter than the reſt of the glaſs.

and moiſture without fell only on the ſurface oppoſite the internal metal, and none on the outer metal;

4. When the humidity was formed both within and without, no moiſture was depoſited either upon the external metal, or its correſpondent place within.

moiſture on both ſides did not wet the external metal; or its place within.

7. When a ſmaller rectangle was paſted on the outer ſurface in the middle of the place correſponding with the interior rectangle, and humidity was depoſited internally, the interior rectangle was not entirely wetted as it would elſe have been, but the place correſponding with the ſmall external rectangle remained dry.

A ſmall external metal on the middle of the ſurface oppoſite the internal metals, produced dryneſs on the oppoſ. metal.

8. If upon the ſmall external rectangle a diſc of glaſs be applied, a round wet ſpot is formed within in the middle of the dry place of which we have juſt ſpoken, correſponding to the place occupied externally by the glaſs.

A ſmall external glaſs plate in the middle of the external metal, produced wetneſs within.

If the whole external metal be covered with glaſs, its effect ceaſes.

9. If inſtead of this laſt mentioned ſmall diſc, a glaſs of the ſame ſize as the external metallic diſc be applied ſo as exactly to cover this laſt, the humidity is diſcovered over the whole of the interior rectangle in the ſame manner as if the glaſs were bare on the outſide.

Metal upon this laſt glaſs plate revives the effect.

a. If upon the rectangle of glaſs there be applied another metallic rectangle, no humidity is again produced on the correſponding interior ſurface, though it continues to be formed on the ſame ſide on the reſt of the metal and glaſs.

Another glaſs deſtroys it.

b. But the humidity is again condenſed, if on the laſt external metallic rectangle there be applied a rectangle of glaſs.

Another metal revives it, &c.

c. And this again diſappears, or ceaſes to be produced, by the application of a third metallic leaf.

d. A third rectangle of glaſs cauſes it again to appear.

e, *f*, *g*, &c. &c. &c.

This alternation is limited.

Neverthelеſs there is a term at which the phenomenon becomes irregular; and this is when the whole thickneſs of the metallic baſes and plates of glaſs amounts to 16 or 20 millimetres (three quarters of an inch).

10. Though the above phenomena clearly prove that the metal does not act as a defence or cover in the production of theſe effects, the author proves it ſtill more ſtrongly in the following manner.

Gilt paper operates like a metal if the gold be outſide; otherwiſe not.

He applies to a pane of glaſs gilt papers of the ſame ſize and figure, ſome by their metallic faces, and others by the ſurfaces which are uncovered, all the other circumſtances being the ſame. Thoſe papers only of which the metallic ſurfaces were expoſed to the air produced the effects of metals, and the others produced no effect. This experiment repeated with pieces of glaſs coated with tin foil inſtead of gilt paper, produced the ſame reſults.

Inſide metal at a diſtance from the glaſs cauſes condenſation on the glaſs, and not the metal.

11. If in the inſide of a chamber, and parallel to one of the panes of glaſs in a window, there be placed a diſc of metal of ſix or ſeven centimetres in diameter (two inches and a quarter) ſo that it ſhall be ſupported only by the middle, and the face ſhall be only a few millimetres (or twenty-fifths of an inch) diſtant from the glaſs, when the humidity comes on within there is none depoſited on the diſc, unleſs it be extremely near, but on the oppoſite glaſs it is depoſited in much greater quantity than any where elſe.

12. If inſtead of this diſc of metal a diſc of glaſs be ſubſtituted, the humidity is not accumulated oppoſite this diſc any more than on the reſt of the glaſs.

Glaſs inſtead of metal has no effect.

13. The inverſe phenomenon takes place on the outer ſurface under like circumſtances.

Outſide, the reverſe.

14. If the metallic diſc (of ſection II.) be varniſhed on the ſide which faces the interior of the chamber, the phenomenon takes place in the ſame manner.

Varniſh on the face fartheſt from the glaſs has no effect

15. If the ſide next the window be varniſhed, or if both ſides be varniſhed, the phenomenon does not take place.

on the ſide next the glaſs, deſtroys the effect.

16. Theſe properties of the metals belong alſo to other conductors of electricity, but with ſome modifications according to their nature.

Other plates as well as metals.

17. If a plate of metal be ſubſtituted inſtead of a ſquare of glaſs, humidity is ſometimes produced, but always much leſs than upon glaſs.

Metal plate inſtead of the pane of glaſs.

18. If this plate of metal be thin and flat, and a diſc of glaſs be faſtened to it, the humidity is produced in preference, or more abundantly on the correſponding interior ſurface.

Pane of glaſs on a plate of metal.

19. All theſe phenomena take place in the ſame manner, or nearly ſo, when panes of glaſs fitted up with the metallic diſcs, or apparatus before deſcribed, are ſupported upon ſhort pegs in the middle of a field where the graſs has been mowed and clipped; in theſe circumſtances the upper ſurface of the glaſs repreſents the outſide, and the lower the inſide of the chamber.

The ſame phenomena in the open air, where the ground repreſents the chamber, and the upper ſpace. the outer air.

20. The interior humidity may be excited provided the weather be rather cold, by means of a ſtove, or by ſprinkling water on a red hot body; and in the country during dry weather, the ſpace on which the glaſſes are to be expoſed may be lightly watered after ſun-ſet.

Artificial production of dew in a chamber; and u doors.

21. If different liquids ſuch as water, mercury, alcohol, oils, acids, &c or even ſmall leaden ſhot be put into glaſs veſſels equal and alike in all reſpects to the half, or two thirds of their height, and they be expoſed to the dew out of a window, the humidity will not be depoſited on the lower part of the veſſel, but only above the level of the ſubſtances contained, and at a diſtance which varies according to the nature of theſe ſubſtances, being greater for mercury than for water, greater for water than for oil, &c.

Other bodies different from metals.

22. In all theſe phenomena when the humidity is too abundant, the reſults become confuſed and uncertain.

The humidity muſt be moderate.

Difficult to ſhew whether the metals differ.

23. The author made ſeveral experiments to aſcertain whether ſome of the metals might not be better adapted to the production of theſe phenomena than others. Having, for example, expoſed to the dew plates of copper gilt and ſilvered, and plates of ſilver gilt only on the half of one of their faces, he obſerved nothing deciſive. It only appeared to him, that in general the drops were formed larger on the white metals. But theſe comparative experiments are difficult to make on account of the readineſs with which moſt of the metals become oxided in moiſt air; which then gives more or leſs of the property of glaſs.

Differences of temperature on the two ſides.

24. In making theſe obſervations Cit. Prevoſt almoſt always kept account of the changes produced in the temperature, or the difference between the internal and external temperature. He endeavoured to procure thermometers with flat bulbs, ſufficiently ſenſible to point out the difference of temperature of the two ſides of the glaſs at the ſame inſtant, which he thinks would have afforded inſtructive reſults; but he has not yet ſucceeded. He only remarks that it is not neceſſary that the external temperature ſhould, as is commonly thought, be more elevated than the internal, in order that humidity ſhould be formed on the outſide of the glaſs, but that the contrary often happens.

Farther reſearches.

25. Theſe reſearches may be carried farther, for example, by making ſimilar experiments in vacuo, or in other gaſes as well as air, and by employing other liquids inſtead of water.

26. In order to reduce the principal facts contained in his memoir to a ſmall number of propoſitions, Cit. Prevoſt gives the name of the *armour of contact* to a metallic leaf applied or glued againſt the glaſs, and *armour of diſtance* to that which is ſome millimetres off from it. This being premiſed, the following are the general facts:

General ſtatement of the facts.

Armour of metal on the warm face condenſes moiſture whether without or within;

I. When a partition or frame of glaſs, which ſeparates two great maſſes of air at unequal temperatures, is armed in contact on its *warm face*.

A. If humidity be depoſited on this face, it accumulates on the armour, inſomuch that there is more there than elſewhere.

B. And if humidity be depoſited on the oppoſite face, or the *cold face*, it accumulates on this face in the place correſponding with the armour, ſo that there is more there than elſewhere.

C. The ſame happens when the armour is at a diſtance; but then if the humidity be depoſited on the warm ſide, it is not on the armour, but it accumulates oppoſite to it upon the glaſs.

on the glaſs if the armour be diſtant.

II. When the partition is armed in contact with the cold face.

Armour on the cold face repels moiſture on both ſides,

A. If humidity be depoſited on this face, it is not on the armour.

B. And if humidity be depoſited on the oppoſite face, that is to ſay, the warm face, there is none on the correſpondent place of the armour.

C. The ſame happens when the armour is at a diſtance on the cold face. But then if the humidity be depoſited on this face, there is not only a want of it on the armour, but there is none on that ſurface of glaſs oppoſite the armour.

if at a diſtance.

III. A. In order that the phenomena ſhould ceaſe to take place, it is ſufficient that the armours of contact, or the face of the armours of diſtance oppoſite the glaſs, ſhould be covered with glaſs or varniſh. Nothing remarkable then happens.

Glaſs or varniſh prevents the effects of the armour.

B. If this glaſs or varniſh be again covered with metal, the phenomena again occur.

Alterations of glaſs or metal.

C. They ceaſe if this metal be again covered with glaſs.

D. They begin, &c.

IV. One ſingle propoſition will include almoſt the whole of theſe facts.

When glaſs ſeparates two maſſes of air of unequal temperatures, the armour gives it (or ſeems to give it) the property of accumulating or repelling humidity, according as it is applied either on the warm *or* cold *face, and this influence extends through the glaſs and through other ſubſtances, to the diſtances of ſeveral centimetres.*

General propoſition or ſtatement.

V. Or ſtill more ſimply.

Glaſs which ſeparates two maſſes of air of unequal temperatures, accumulates (ſeems to accumulate) or repells humidity, according as it is armed on the warm or cold face.

Simple general law of thoſe facts.

EXPLANATION.

I imagined for a long time, ſays the author, that theſe phenomena depended on electricity, but they may be explained very naturally by the elective attractions which are exerciſed at a diſtance, and by the well known property of the metals of being good conductors of caloric.

BASES

BASES OF THE EXPLANATION.

eneral baſes r explaining em.

1. *The leſs the temperature of glaſs is elevated, the more humidity it attracts from the air.*

2. *Metals attract it very little.*

3. *Glaſs ſenſibly exerciſes its action on the humidity of air, at a diſtance, and notwithſtanding the interpoſition of different bodies, ſuch as plates of metal,* &c.

4. *Metals give to glaſs, near which they are placed, the property of more ſpeedily attracting caloric from hot air, and on the contrary, that of yielding it more ſpeedily to cold air.*

N. B. When I ſay that metals give glaſs this property, I mean that *they act as if they gave it them;* which is evident by an examination of two thermometers, one of mercury the other of alcohol, which are plunged at the ſame time in air either colder or warmer than that whoſe temperature they indicate. The metallic thermometer arrives much ſooner than the other to that of the new medium. Its glaſs then, if colder, muſt take up more ſpeedily from the medium the caloric which it tranſmits to the metal, or, if hotter, it muſt more ſpeedily give out that of the metal.

emarks:

The firſt baſis has been long eſtabliſhed.

The ſecond and third are proved by the 17th and 18th ſections of the extract.

The laſt is a neceſſary conſequence of the conducting property of metals.

This being premiſed, it is eaſy to comprehend that,

he laws applied to explain ll the facts.

A. When the glaſs is armed on its warm face (§ 27, 1ſt, A. B. and C.) it yields its caloric to the cold air more ſpeedily than that which is not armed (*baſis* 4), and conſequently it attracts humidity more powerfully (*baſis* 1), whether directly on the glaſs, or through the metal, or on the metal itſelf (*baſis* 3), if this be in contact; but if it be at the diſtance of ſome millimetres, the humidity not meeting the metal on its paſſage, accumulates on the oppoſite glaſs in a greater quantity than elſewhere.

B. If the metal be applied on the cold ſide (§ 27, II. A. and B.) the glaſs moſt heated does not attract ſo much humidity (*baſes* 4 *and* 1), and it accumulates on the unarmed part of the pane.

C. If, in this caſe, the armour be covered with a plate of glaſs, the plate cools more ſpeedily than if the metal were not preſent;

ſent; but as the partition is more heated than if it were not preſent, there is no effect, and the totality of the double glaſs armed within, is in the ſame caſe with that unarmed; it therefore accumulates neither more nor leſs humidity.

A ſecond armour on the plate of glaſs will cauſe the phenomena to re-appear; a ſecond plate of glaſs on this new armour will again make it diſappear, &c. For as long as the ſymmetrical glaſs ſhall have the interior armours, the cauſes of heat and cold will be found in equilibrio; but an additional armour will neceſſarily deſtroy the balance, and the heated glaſs will not attract humidity; which explains the facts of § 27. III. A. B. C. D. &c.

D. If the glaſs be armed on both ſides (§ 7), as it would not then be expoſed to the air, either on the cold or warm ſide, it ſeems that it ought to attract as much humidity on the armours as on the reſt of the partition. But though the glaſs exerciſes its action through the metal, this is nevertheleſs an obſtacle which diminiſhes its force; humidity in this caſe will not then be ſo ſtrongly attracted by the doubly armed glaſs as by the part perfectly unarmed, &c.

CONCLUSION.

Theſe obſervations are not only intereſting, but they appear **Concluſion.** to eſtabliſh an important point in philoſophy; namely, that *glaſs exerciſes its attraction for the humidity (which has a tendency to be depoſited from the air) through metals.* See likewiſe § § 19 and 26 for the generalization of this propoſition.

EXPLANATION OF THE FIGURES.

Fig. 1, Plate XV. relates to Art. 4 of the abſtract. A is **Explanation of the figures.** the interior rectangle more deeply ſhaded than the reſt of the ſquare, which ſignifies that it is more moiſtened. B is the place correſponding to the exterior rectangle, with its ſurrounding part repreſented white, becauſe dry.

Fig. 2 relates to § § 7 and 8.

Fig. 3, 4, 5, 6, 7, relate to Art. 7 and 9. I c ſignifies *interior ſide hot;* E f, *exterior ſide cold.* The lines more deeply ſhaded repreſent the ſection of the metallic leaves; the white lines repreſent the ſection of the glaſs, and the dotted ſhades repreſent drops of dew or moiſture.

Fig. 8 relates to Art. 11.

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Abſtract of the late Experiments of Profeſſor ALDINI *on Galvaniſm.*

Galvanic experiments of Aldini.

M. ALDINI, Profeſſor at the Inſtitute of Bologna, and nephew of the celebrated Galvani, after having made his experiments at the National Inſtitute of France, has viſited London, and given an accurate account of his experiments and diſcoveries to the Royal Society, before whom the ſame was read on the 25th laſt. I have the pleaſure to communicate ſome of the principal facts which he has had the goodneſs to communicate to me, which appear calculated to throw much light on ſome of the moſt difficult phenomena of nature.

The theory that electricity is generated in animals,

verified by Aldini;

by ſubſtituting part of an animal inſtead of the metallic pile.

The head of an ox convulſes part of a frog.

Various philoſophers have conſidered the metals as not abſolutely neceſſary for the production of galvaniſm, and Mr. Davy has proved it in the pile: It has alſo been indiſtinctly apprehended or conjectured in the way of theory, that the galvanic or electric matter was excited, collected, or generated in the bodies of animals, where it was conſidered as the great cauſe or inſtrument of muſcular motion, ſenſation, and other effects highly intereſting, but very little underſtood. Profeſſor Aldini has the diſtinguiſhed merit of having placed this propoſition in the rank of eſtabliſhed truths. He has ſucceeded in exciting muſcular contractions by the ſimple application of the nerves to the muſcles of a prepared frog, without the leaſt ſuſpicion of any ſtimulus ariſing from contact. He has alſo given motion to the limbs of a ſmall cold blooded animal by the galvanic energy of an animal with warm blood; an experiment never before imagined. He takes the head of an ox recently cut off, and applying the finger of one hand wetted with ſalt water to the ſpinal marrow, he holds in the other hand the muſcle of a frog prepared (that is by diſſection) in ſuch a direction that its crural nerves ſhall touch the cervical muſcles on the tongue of the ox. Every time of this contact ſtrong contractions are produced in the frog. If a chain of ſeveral perſons be formed holding hands, the ſame effect takes place; but the contacts do not produce any effect if the chain of connection be broken or interrupted. Here then we have the moſt decided ſubſtitution of the organized animal ſyſtem in the place of of the metallic pile: it is an animal pile; and the production of the galvanic fluid, or electricity, by the direct or independent energy of life in animals, can no longer be doubted.

The Profeſſor has lately repeated the ſeries of theſe experiments at Oxford, and ſhewed in the preſence of Doctors Pegg and Bancroft, that the nerves of a prepared frog, diſpoſed in the manner here ſtated, approach very ſenſibly to the muſcles of the warm-blooded animal, and exhibit a real attraction hitherto unknown in natural philoſophy and phyſiology. He invites philoſophers to vary and repeat this phenomenon, which has been confirmed by different philoſophers, particularly by the celebrated Felice Fontana of Florence. Galvaniſm by theſe facts, is ſhewn to be animal electricity, not merely paſſive, but moſt probably performing the moſt important functions in the animal economy. This power appears not to be confined in its operation to the motion of the muſcles, but alſo appears to be of importance in the ſecretions. Aldini has given ſtrength to this conjecture, by ſubjecting the urine to the power of the artificial galvanic ſtream, and he found it productive of a ſeparation of the principal combined parts of that fluid, which were conſidered as of much importance by the celebrated Profeſſors Senebier and Jurine, of Geneva.

Theſe experiments repeated at Oxford.

Animal attraction,

verified.

Galvaniſm operates in the ſecretions.

A very ample ſeries of experiments were made by Profeſſor A. which ſhew the eminent and ſuperior power of galvaniſm beyond any other ſtimulant in nature. In the months of January and February laſt, he had the courage to apply it at Bologna to the bodies of various criminals who had ſuffered death at that place, and by means of the pile he excited the remaining vital forces in a moſt aſtoniſhing manner. This ſtimulus produced the moſt horrible contortions and grimaces by the motions of the muſcles of the head and face; and an hour and a quarter after death, the arm of one of the decollated bodies was elevated eight inches from the table on which it was ſupported, and this even when a conſiderable weight was placed in the hand. Theſe experiments have ſince been confirmed in various parts of Italy, particularly at Turin, by Profeſſors Giulio, Vaſſali, and Roſſi.

Aſtoniſhing effects on human ſubjects.

Contortions of the muſcles, &c. of the face:

the arm lifted up.

Theſe are not experiments of pure curioſity, but offer very encouraging proſpects for the benefit of mankind, in diſorders of the head, and in apoplexies. Profeſſor Aldini means to apply

Theſe experiments promiſe great benefits in the cure of diſorders.

part of his time in London, in communicating theſe important ſubjects of information to phyſicians, as he has already done in Paris, in which place he made ſome applications of his diſcoveries, chiefly at the Hoſpital de Salpetriere, in company with Doctor Pinel*. The application of galvaniſm in melancholic inſanity is abſolutely new, and of great intereſt. He perfectly cured two patients at Bologna of this diſorder; and on that account he is more deſirous of recommending the trial in an affliction ſo diſtreſſing, againſt which the preſent ſyſtem of phyſic has ſo little to offer.

Inſanity cured.

Apoplexy.

Recovery of the drowned.

Reſpiration ſtrongly excited by galvaniſm.

Precautions.

Profeſſor Aldini intends to publiſh a large work.

It appears to be equally promiſing in caſes of apoplexy. Aldini thinks it may be highly uſeful in recovering drowned perſons, and on that account he is deſirous of communicating with the Society eſtabliſhed at London for the recovery of theſe unfortunate individuals. An experiment lately made at Paris adds much to his expectations. At the Hoſpital de la Charité he ſhewed the pupils, that galvaniſm applied to the trunk of a dog, to the ſpinal marrow, and the inteſtines, cauſed the lungs to act in ſuch a manner that the air that iſſued from the aſpera arteria twice in ſucceſſion, extinguiſhed a large candle placed oppoſite. Now, as in moſt caſes little more is required in drowned ſubjects, than to ſet the organs of reſpiration into action, it is to be preſumed that the action of galvaniſm may be of the higheſt utility in theſe caſes. Many precautions are neceſſary to be uſed in the adminiſtration of this powerful remedy in lunacy or apoplexy, which will be detailed in a large work which Profeſſor Aldini intends to publiſh in this country before his return to Italy. In the mean time the reader will doubtleſs receive ſatisfaction from this ſhort notice he has enabled me to give of his labours on a ſubject which promiſes greatly to extend the limits of natural ſcience, and may be reaſonably expected to add to the powers which man is enabled to exert for his own benefit over the numerous beings around him.

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* Whoſe *Treatiſe ſur la Manie* is well known in this country.

"Examen, &c." *Chemical Examination of a New Gas, compoſed of Hidrogen, Carbon, and Phoſphorus.* *By* J. B. TROMMSDORFF.

A new gas by Trommſdorff.

Mr. Trommſdorff obtained this gas during the decompoſition of phoſphoric acid by ignited charcoal. In its common ſtate it is mixed with carbonic acid, which may be ſeparated from it by agitation in lime water.

The new gas is nearly of the ſame ſpecific gravity as common air; it is inſoluble in water, and undergoes no change when mixed with oxigen, at common temperatures; but it detonates with that aëriform fluid by the action of heat. It is poſſeſſed of no agency upon the ſolutions of metallic oxides which are not reducible by heat, but it decompoſes the fluid ſaline compounds containing gold, ſilver, or mercury. During its combuſtion with oxigen, water, phoſphoric acid, and carbonic acid are formed, and hence Mr. Trommſdorff is inclined to conclude that it is a triple compound of phoſphorus, hidrogen, and carbon; and he propoſes to call it by a name, which may be tranſlated by the term of phoſphorated carbonated hidrogen gas.

A part only of the memoir from which this account is taken is as yet publiſhed. Concerning the action of the new gas upon metallic ſolutions, and other phenomena preſented by it, the learned author promiſes to enter upon ſome additional details. Without wiſhing to anticipate any of his reaſoning upon the ſubject; with the ſimple hope of throwing out a hint for future diſcuſſions, we ſhall venture a general obſervation or two in relation to it.

If hidrogen exiſts in the gas, it muſt apparently ariſe from the decompoſition of water contained by the concrete acid or the charcoal; for, as would appear probable from the experiments of Deſormes and Clement, well burnt charcoal contains no aſcertainable quantity of combined hidrogen. By ſeparately igniting the phoſphoric acid and the charcoal, before they were made to act on each other, the water contained by them would be driven off; and, under ſuch circumſtances, it would be curious to aſcertain if the gas of Mr. Trommsdorff would be produced.

As phoſphorus had a very ſtrong affinity for oxigen, we ſhould be diſpoſed, *a priori*, to conclude, in reaſoning upon the facts lately diſcovered concerning the gaſeous oxide of carbon, that this ſubſtance would be formed and evolved with the carbonic acid in the proceſs of the decompoſition of phoſphoric acid by charcoal. Is it not poſſible that the new gas may be a mixture of a triple compound of carbon, phoſphorus, and oxigen, with carbonated hidrogen produced from the decompoſition of water united to the primary ingredients? There is nothing in the experiments detailed in the memoir which militates againſt this ſuppoſition, and it might be ſubmitted to the proof of a new experiment at the ſame time with the theory of the author.

D.

Notice concerning certain peculiar Habits of the Shark and Pilot-Fiſh, by GEOFFROY, *Profeſſor at the Muſeum of Natural Hiſtory.*

Intereſting narrative of ſhark and pilot fiſh.

IT is aſſerted, that the ſhark has brought under his command, a very ſmall fiſh of the ſpecies of the gadus, that this latter fiſh precedes its maſter in its progreſs, that it points out thoſe parts of the ſea which moſt abound in fiſh, and the method of obtaining the prey of which he is moſt greedy; and that the ſhark, notwithſtanding its extreme voracity, in acknowledgment for theſe ſignal ſervices, lives on good terms with this uſeful companion. Naturaliſts, who are always guarded againſt the exaggerations of travellers, and not being able to account for ſuch an aſſociation, have doubtfully rejected theſe facts. It will be ſeen that they are wrong in ſo doing: the obſervations which I have made are accompanied by circumſtances, which perhaps were never preſented to any one but myſelf in ſo detailed a manner.

The 6th Prairial in the year 6, I was on board the frigate *Alceſte* between Cape Bon and the Iſland of Malta. The ſea was calm: the paſſengers were tired with its long duration, when their attention was arreſted by a ſhark which they perceived approaching the veſſel. It was preceded by pilot fiſh, who were nearly at the ſame diſtance from one another as they

Intereſting narrative of ſhark and pilot fiſh.

were from the ſhark: the pilot fiſh approached the poop of the veſſel, they examined it twice from one end to the other, and being ſatisfied that there was nothing there to anſwer their purpoſe, they continued the courſe they had formerly held. During theſe different movements, the ſhark never loſt ſight of them, or rather he followed them ſo exactly, that it ſeemed as if he had been drawn along.

As ſoon as they were diſcovered, a ſailor immediately prepared a large fiſh hook, which he baited with pork; but the ſhark and his companions, by the time the fiſher had got ready, were at the diſtance of 20 or 25 metres; he nevertheleſs threw his bait into the ſea. The noiſe occaſioned by the fall was heard at a diſtance. The fiſh were aſtoniſhed and ſtopped their courſe; the pilots immediately turned, and prepared again to examine the poop of the veſſel. The ſhark, during their abſence, amuſed himſelf in a thouſand different ways at the ſurface of the water: he turned on his back, then on his belly, dived into the ſea, but always appeared again in the ſame place. The pilots having arrived at the poop of the *Alceſte*, paſſed near the pork, and as ſoon as they perceived it, they returned to the ſhark with greater ſwiftneſs than when they left him. When they had reached him, he began his courſe: the pilots ſwimming one on his right and the other on his left, exerted themſelves to keep before him; they with difficulty did ſo, and returned a ſecond time to the poop of the veſſel; the ſhark followed them, and owing to the ſagacity of his companions, perceived the prey that was deſtined for him.

It has been ſaid that the ſhark has a very powerful ſmell: I paid much attention to what paſſed when he was near the pork: it ſeemed to me that he did not perceive it till the very moment that his guides, as it were, pointed it out to him; he did not till then ſwim with greater ſwiftneſs, or rather make a ſpring to ſeize it. He firſt ſeized a portion without being hooked; but at the ſecond attempt the hook pierced the left lip: he was hoiſted on board.

At the expiration of two hours, during which I was occupied in the anatomy of this ſqualus, I began to regret that I had not more nearly examined this ſpecies, which ſo willingly conſecrates itſelf to the ſervice of the ſhark: I was aſſured that I might eaſily do it, as it was certain that they had not quitted the neighbourhood of the veſſel, and ſome moments after they pointed one out to me, which I perceived was of the ſpecies of pilot fiſh of the ſailors, and of the *gaſteroſteus ductor* of naturaliſts.

It would doubtleſs be curious to examine what intereſts can induce animals ſo different in their organization, their ſize and habits to form ſuch an intercourſe. Does the pilot fiſh feed on the dung of the ſhark as Cit. Boſc has ſuppoſed; or does it impoſe on itſelf the painful duties of ſervitude, in order to find protection and ſafety in the company of ſo voracious a ſpecies.

Notice of a New Metal, by Profeſſor PROUST.

w metal
mes.

Profeſſor Prouſt, in a letter to Dr. Delametherie, promiſes to ſend an account of a new metal he has lately diſcovered in an ore from Hungary. He is not yet acquainted with its metallic aſpect, he is apprehenſive it will not be eaſily reduced, from its retaining oxigen with conſiderable avidity, and like many other metals, it is ſuſceptible of two degrees of oxidation: the ſolutions of its oxide at maximum is yellow, green at the minimum, when in theſe two ſtates it colours glaſs; laſtly, it may be ranked in the claſs of thoſe metals which retain oxigen againſt ſulphurated hidrogen. He has purified it by the ſame means he employed with nickel, cobalt, iron, manganeſe.

J. de Phyſ. Vendemaire, an 11.

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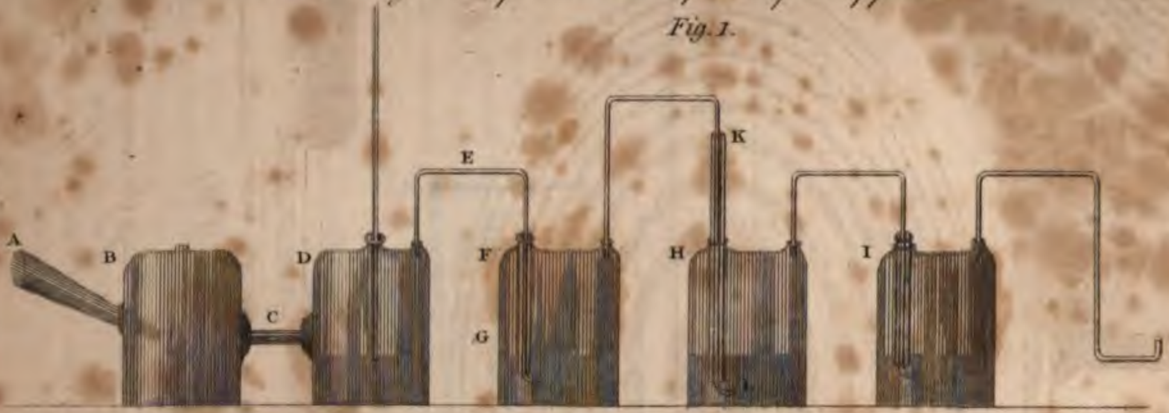
Philos. Journal Vol. III. Plate XIII. p. 304.

Mr. Murray's Improvement of Woulfe's Apparatus.

Fig. 1.



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Philos Journal Vol. III. Plate XIV p. 304

Dr. Young's Apparatus for exhibiting the Colours of thin plates.

Fig. 1.

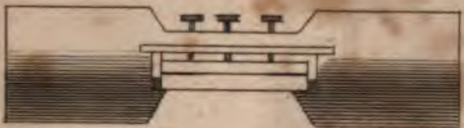
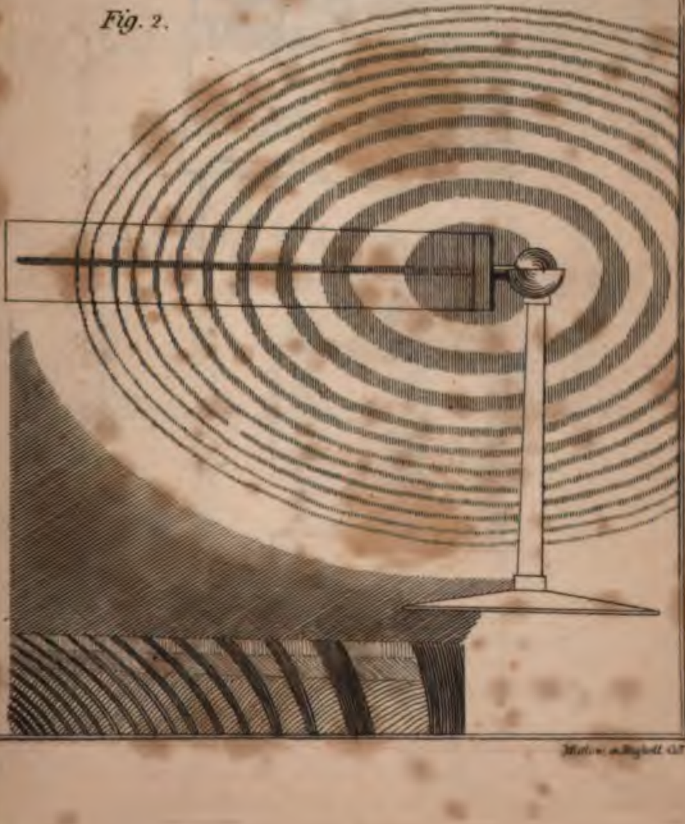


Fig. 2.



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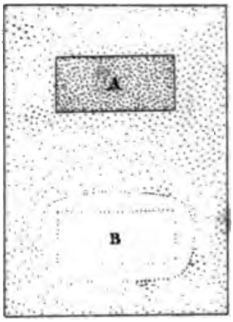
Prevost on the Phenomena of Dew.

Fig. 1.



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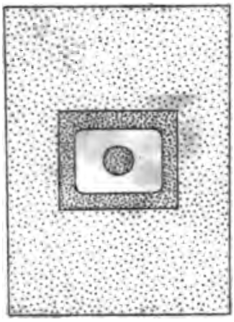

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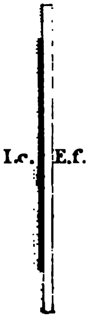

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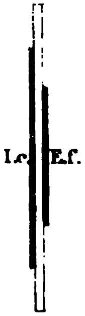


Fig. 5.

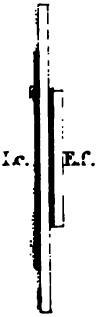


Fig. 6.

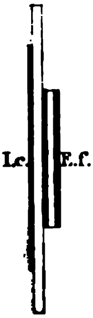


Fig. 7.

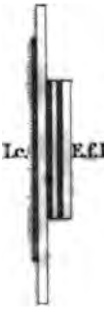
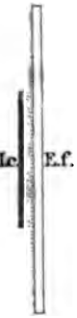


Fig. 8.



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VOL. IV.

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BY WILLIAM NICHOLSON.

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1803.

PREFACE.

ON the Completion of this fourth Volume I have the Pleasure to give the Names of the Authors and List of Engravings.

The Authors of original Papers are John Gough, Esq.; N. N.; J. Fletcher, Esq.; Ez. Walker, Esq.; J. Harriott, Esq.; Mr. W. Wilson; Dr. Priestley, F. R. S. &c.; Thomas Young, M. D. F. R. S. &c.; Mr. Benj. Hooke; W. N.; John Bostock, M. D.; G. C.; R. B.; The Rev. Wm. Gregor; Mr. Wm. Henry; Z.; Π; and Lawson Huddleston, Esq.—Of foreign Works, G. Dalarive; Rochon; Guiton; Cadet; Gall; Bojames; Klaproth; Jumilhac; Saint Victor; J. B. Berard; L. Reynier; Proust; and Fourcroy.—And of English Memoirs abridged or extracted, R. Chenevix, Esq. F. R. S.; H. Davy, Esq.; S. Tennant, Esq. F. R. S.; Mr. J. Dalton; W. H. Wollaston, M. D. F. R. S.; W. Herschel, L. L. D. F. R. S.; R. Kirwan, L. L. D. F. R. S.; and M. des Lozieres.

Of the Engravings the Subjects are, 1. A new sliding Stop for Air-Pumps and other Uses. 2. A simple and secure Metallic Joint for Tubes. 3. Mr. Harriott's Syphon Engine for raising and lowering Weights. 4. New Method of measuring the Action of Bodies in refracting Light, whether they be transparent or opake; by Dr. Wollaſton. 5. Mr. Hooke's Blow Pipe by Alcohol. 6. The Harmonic Sliders of Dr. Young. 7. A strong and flexible Joint for Tubes.

8. Dr. Wollaston's Figures to explain the oblique Refraction of Iceland Crystal. 9. Mr. Dalton's Sketches of the Rivers of England and Wales, and the Districts which supply them with Water. 10. Figure to illustrate Mr. Gough's Theory of sonorous Undulation. 11. The Craniognomic System of Dr. Gall. 12. An Instrument for extracting hard Bodies out of the Œsophagus. 13. A new Reflecting Quadrant by Mr. Ez. Walker. 14. Mr. Huddlestone's Method of floating Boats from one Level to another, without Loſs of Water. 15. Engine for blowing up the Stumps of Trees, by Cit. Saint Victor; and 16. Two Drawings of a Magazine Pistol, which discharges Nine Balls in Succession, by once loading.

Soho Square, April 28, 1803.

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THE ARTS.

JANUARY, 1803.

ARTICLE I.

On the Nature of the Grave Harmonics. In a Letter from Mr. John Gough.

To Mr. NICHOLSON.

SIR,

OUR ſenſe of time ariſes, as Mr. Locke juſtly obſerves, from the conſtant ſucceſſion of ideas in the mind; or to ſpeak perhaps in more intelligible language, it is the reſult of the attention being occupied by an uninterrupted train of changeable perceptions. Time is on this account, capable of increaſe and diminution; it is therefore a ſpecies of the abſtract term, Magnitude; in conſequence of which, the parts of it poſſeſs all the properties contained in the abſtract term, Ratio, and are proper objects of the doctrine of proportion. A great variety of proportion takes place amongſt the conſtituent parts of compound beings of the ſame denomination, and gives birth to a claſs of phenomena in the philoſophy of the human underſtanding; which can only be explained by an hypotheſis aſcribing to man a faculty that compares theſe ratios, and perceives the effect produced by them. Of this deſcription are the differences perceived by the ſenſe of feeling in the texture of bodies,

Time is a ſpecies of magnitude; and its parts have the habitude of ratio to each other.

The mind compares ratios together as well as their parts.

Phyſical effects thus diſcriminated.

dies, the diſcrimination of perſons, which is the office of the eye, and thoſe modifications of time, called ſymphony and harmony, which are judged of by the ear.

The ear affords the conception of a permanent ſound, only when the vibrations are too frequent to be ſeparately conſidered;

but the ratios of frequency are perceived, and excite ideas of acute and grave.

Each ſenſe reckons time, by the train of its own ideas alone; the auditory organ on this account meaſures the lapſe of it by ſound. Now if the ear had the power of conſidering the ſmalleſt intervals of time apart, it would perceive the conſtituent vibrations of ſounds as ſeparate diſtinct things, attracting its notice ſucceſſively; in which caſe, the human mind could not poſſibly form a conception of a permanent ſound: but it is a maxim in harmonics, that a certain number of vibrations in a ſecond is required to give a ſenſible degree of duration to the note of a ſtring; I ſhall at preſent make uſe of 12 for this number. The preceding fact aſcertains an eſſential point of my theory, for it fixes the leaſt interval of time that the ear can contemplate apart; this organ therefore takes in the groſs all magnitudes of this deſcription, which are too ſmall to be examined in detail: in this caſe it acquires the ideas of acute and grave, by comparing two or more trains of vibrations; the intervals of which are equal in each ſeries taken ſeparately, but greater and leſs in the whole number taken together.

Two continuous ſounds, heard together, have their vibrations arranged in cycles of equal duration perceptible by the ear: If the duration be long enough, each cycle is diſtinguiſhed;

if not, the points of recurrence will have the effect of ſimple periods,

and afford a muſical ſound, or grave harmonic,

When two ſounds, thus conſtituted, are heard in concert, the vibrations producing them are arranged in cycles, no one of which continues for a longer or ſhorter time than the reſt; and their effect is perceived by the ear, which becomes ſenſible of their preſence. For when each cycle of a ſeries, ſeparately conſidered, exceeds the twelfth part of a ſecond, the ſenſe of hearing recognizes each point of diviſion made by the coincidence of the vibrations which ſeparate the contiguous cycles: this circumſtance enables the ſenſe to contemplate theſe periods apart, and to comprehend their origin. On the contrary, when the duration of a cycle belonging to a compound ſeries does not exceed the twelfth part of a ſecond, the interval proves too ſmall to be meaſured by the ear; it therefore eſcapes notice in a ſeparate ſtate; for the points of diviſion recur too frequently to be obſerved. When the auditory organ finds itſelf in circumſtances anſwering to the preceding deſcription, it has but one method to purſue; which is to treat theſe derivative intervals in the ſame manner it treats all periods which are ſingly too ſmall for its comprehenſion: it therefore reduces them to a ſimple muſical ſound, correſponding in pitch to a ſtring; which vibrates once in the time of each ſucceſſive cycle: A grave harmonic is on this account always a lower note than any of its conſtituents, ſeeing the time of a cycle exceeds the greateſt vibration that enters into the compoſition of it. The ſtrength of a grave harmonic is alſo weak, when compared with the notes compoſing it, becauſe theſe ſecondary ſounds, being nothing more than certain unavoidable efforts of the imagination, they aſſume the character of a feeble ſound, which is juſt ſtrong enough to be heard in the company of one or more louder tones; for the power of the imagination is always inferior to external impreſſions, except in fits of inſanity, when the organs of ſenſe appear to be blunted by phyſical cauſes. This inferiority of the fancy may be aſſigned as the reaſon of imaginary ſounds, which it frequently creates, being conſtantly faint and apparently diſtant.

lower than either of the two original ſounds, and fainter. I is the reſult of imagination.

Dr. Young mentions a trait of the grave harmonics, which ſpares my own authority, and gives an opportunity to quote his for the following fact, which is of the firſt moment to the preſent theory: The grave harmonics always keep the direction of the ears, let the poſition of the head be changed as oft as you pleaſe, reſembling in this circumſtance a ſhrill piping note, called the ringing of the ears; which every one aſcribes to a ſlight affection of the auditory duct, becauſe it differs from external ſounds in having no fixed direction. The grave harmonics agree with the ringing of the ears in this remarkable particular; which is a ſtrong proof that their immediate cauſe is ſeated in the perſon of the hearer; and it is evident from the nature of things, that this cauſe originates in the mind, ſeeing that the organ does not labour at the time under a phyſical impediment.

In proof of which, the fact *that it has no direction* is urged.

Concluding remarks.

I have now given a general theory of theſe ideal ſounds, in compliance with Dr. Young's injunctions, but am not able to perceive any connection between the ſubject and the queſtion in debate. This was the reaſon why I diſregarded in my laſt, one of the Doctor's remarks, which he has ſince called a challenge, perhaps in a petulant manner: a little more argument and a little leſs ill humour would certainly promote both his cauſe and reputation. The Doctor endeavours to perſuade me, that I miſunderſtand his idea of coaleſcence. My idea of the ſubject is ſimply this, that ſounds cannot coaleſce; conſequently that all compounds are nothing but a number of diſtinct cotemporary ſounds: if the Doctor will condeſcend to ſay, he contends for nothing more, the diſpute is at an end; but ſhould he reject this propoſal, I adviſe him to attempt the refutation of my paper by ſober arguments, and to tranſmit his thoughts to that Society which publiſhed mine; where, without doubt, they will meet with a candid reception.

JOHN GOUGH.

Middleſhaw, near Kendal,
December 13, 1802.

II.

Bagatelles relating to the Pneumatic Apparatus. Sliding Stopcock; ſimple and ſecure metallic Joint for Tubes; Improvement of Read's Apparatus; original Invention of Woulfe's Bottles and Tubes; and of a Method of cloſing Veſſels. By N. N.

ivention of ding ſtop-cks.

§ 1. THE preſent bagatelle writer has been frequently at a loſs, in his pneumatic experiments, to meet with ſtop-cocks both cheap and perfectly air-tight, or to have them eaſily repaired when out of order. In a converſation he had on this ſubject with Dr. Fiſcher, *ci-devant* Aſtronomer at Manheim, he underſtood that Mr. Helfenſrieder, while Profeſſor of Natural Philoſophy at Ingoldſtadt, to remedy ſimilar defects in his air-pumps, uſed ſteel parallelopipedons with a vertical and horizontal opening joined at right angles, and ſliding between braſs-work. This furniſhed the writer with the idea of conſtructing ſliding ſtop-cocks for a gazometer; he was conſtructing ſimilar inſtruments. He lays no claim to the invention; but has found them anſwer extremely well.

eſcription of e cock.

Fig. 1. Pl. I. exhibits a ſection of the ſliding ſtop-cock put together.

Fig. 2. One of the braſs parallelopipedons (*a a*, Fig. 1) againſt which the pieces *b* and *c*, Fig. 1, are preſſed and united by means of four ſcrews.

Fig. 3. The ſlider by which, when brought home on one ſide, the ſtop-cock is opened, and, on the contrary, ſhut when moved fluſh on the oppoſite extremity.

N. B. The bore of the whole is three-eighths of an inch diameter throughout, which is of the greateſt advantage, when any gas is immediately transferred from the diſengaging veſſel to the gazometer; for if the apparatus be ſufficiently air-tight as it ought to be, and the gas rapidly diſengaged, an exploſion may be occaſioned for want of a ſufficient paſſage for the gas.

Fig. 4. The piece *c*, Fig. 1. Its companion *b* ends in a male ſcrew to be united to the gazometer.

Fig. 5. A perſpective view of the ſtop-cock, the above enumerated parts being put together.

Fig. 6. A nut provided with a female ſcrew ſoldered to the gazometer, by means of which the ſtop-cock may be applied or removed as uſual.

Fig. 7. repreſents two braſs cylinders. One end *f* is fitted by grinding to the orifice of the ſtop-cock; and to the other ends *g* and *h*, between *x*, is faſtened a flexible tube for experiments with the blow-pipe.

Cheap and very uſeful flexible tube for the gaſes.

This flexible tube was made by twiſting a braſs-wire ſpirally round a long thin cylinder, covering this with oiled ſilk twice wrapped round, and faſtened by means of thread between the grooves of the wire. It was then again varniſhed and covered in a ſpiral manner with ſheep-gut ſlit longitudinally, and again ſecured with thread. Laſtly, to protect the whole from external injury, it was covered with leather in the ſame manner as the tubes of the inhalers. Theſe flexible tubes anſwer the ſame purpoſe as the very coſtly ones made of elaſtic gum, ſimilar to the hollow bougies made for ſurgeons.

Fig. 8. A (broken) blowpipe, put in this place merely to ſhew how its end *k* is connected with the gazometer, by fitting into the end (*f*) of the braſs end of the flexible tube.

Deſcription of joint for metall tubes.

§ 2. In pneumatic experiments for transferring any gas, for inſtance oxigen, from the air-holder to another veſſel, it is frequently neceſſary to have tubes which can be joined in certain directions and inclinations; and for this purpoſe a contrivance has been adopted, which is well known, and is repreſented Fig. 1. Plate II. where *a* and *b* are ſpherical ſegments, and *c* their junction by means of a ſcrew. But as this apparatus, beſides that its parts are with difficulty well ground together, is liable to become imperfect, more eſpecially by ſtrong preſſure, or a blow received on the edges of the ſegments, or a grain of duſty ſand interpoſed between the ſmall flat ſurfaces, the writer of theſe trifles has contrived another,

A better conſtruction.

delineated Fig. 2. A B C are the parts correſponding to thoſe of the preceding. It ſerves as a connecting piece, to which tubes at D and H may be joined in the requiſite directions. Its conſtruction and advantages are ſufficiently obvious from the mere inſpection of the drawing, ſo that nothing is requiſite to be added on this account. Several friends had made it after this model, and highly recommend its uſe: and ſince the making of it the writer underſtands that the ingenious Mr. Webſter, whoſe merits were never properly eſtimated by the great improver of culinary utenſils, had the ſame thought, and executed it about two years ago; he, therefore, does not claim the merit of its firſt invention, but only (perhaps) that of publiſhing it.

The ſliding ſtop-cocks, § 1, as well as theſe connecting pieces, § 2, have been executed in a maſterly manner by Mr. Hooke, optician and mathematical inſtrument maker, No. 159, Fleet-ſtreet.

Various improvements in Read's funnel apparatus.

§ 3. In Mr. Read's very ſimple and cheap pneumatic apparatus (ſee Nicholſon's Journal, new ſeries, Vol. III. page 55) two improvements have ſince been made. The firſt, that the top of the exterior tube is not ſoldered to the funnel I (Plate IV. *cit.)* but has a top ſcrewed on with a collar of leathers, and the upper part of the innermoſt tube A F projects above it 1½ inch, and receives the braſs cylindrical end of the funnel, fitted to it by grinding. The ſecond improvement has been made by Mr. Hooke, who inſtead of ſoldering all the parts together, has joined the interior parts to the outer tube A A, in the middle about E. By theſe means, if any impurities that might impede the action of the inſtrument, ſhould happen to ſettle at the bottom C or D, or at L, the whole may be eaſily taken aſunder and cleaned.

Woulfe's apparatus was invented by Glauber.

§ 4. The whole chemical world ſpeaks, hears, and writes of Woulfe's pneumatic apparatus. The tribute of merit muſt not be denied to Mr. Woulfe; but perhaps it may be curious to know, that the original invention of this contrivance, in the ſtrict ſenſe, belongs to good honeſt father John Rudolph Glauber. If any gentleman doubt of this fact, let him conſult Glauber's works, tranſlated into Engliſh by Chriſtopher Packe, and printed, Lond. 1689, in folio, for the tranſlator, by Thomas Milbourn. The very firſt plate to Glauber's Treatiſe on Philoſophical Furnaces ſhews it, Fig. 3.

§ 5. An-

Another old invention.

§ 5. Another bagatelle of this kind, relating to pneumatic inſtruments, is a pretended new method of cloſing bottles and other veſſels air-tight *without luting or grinding*, which, a few years paſt, I ſaw recommended (I do not recollect in which of our Journals), and which conſiſts in having a groove round the neck into which a cap fits, ſee Fig. 3; ſo that this groove *(a)* may be charged with water or mercury as circumſtances require. This likewiſe belongs originally to Glauber, as may be ſeen in the plate juſt before quoted of the tranſlation of his works.

There is *nothing new under the ſun*, ſays Solomon, *Eccles.* I. v. 10. and more of this kind of *old news* ſhall be occaſionally given by,

SIR,

Your humble ſervant,

N. N.

December 12, 1802.

III.

Analyſis of Corundum, and of ſome of the Subſtances which accompany it; with Obſervations on the Affinities which the Earths have been ſuppoſed to have for each other in the humid Way. By RICHARD CHENEVIX, *Eſq. F. R. S. and M. R. I. A. From the Philoſophical Tranſactions for* 1802.

§ I.

Kinds of corundum not examined by Klaproth.

SOME kinds of corundum, ſuch as the adamantine ſpar of China, and the ſapphire, have already been analyzed by Mr. Klaproth. This would have rendered any further experiments unneceſſary, were it not, that I have had at my diſpoſal many kinds of corundum he did not poſſeſs, and alſo ſome ſubſtances accompanying it, which were unknown before the preceding communication of the Count de Bournon.

All the kinds are ſimilarly conſtituted.

As, from the reſult of my analyſes, it appears that all the different kinds of corundum are nearly ſimilar in their conſtituent parts, and differ only in their proportions, it would be tedious to mention every experiment I made upon each kind. I ſhall therefore confine myſelf to ſtating, once for all, ſuch modes of analyſis as were employed with ſtones of a ſimilar nature; and then preſent a ſummary of the reſults: laſtly, I ſhall conclude with an enquiry into a much conteſted point, which lately threatened a revolution in docimaſtic chemiſtry.

Principal character: Extreme hardneſs.

A principal character of corundum in general, as may be found in the Count de Bournon's mineralogical deſcription, is extreme hardneſs; and thence the difficulty of reducing that ſubſtance into fine powder will be eaſily conceived. We are told by docimaſtic chemiſts, that the moſt advantageous method of pulverizing hard ſtones, is to make them red-hot; and, in that ſtate, to plunge them into cold water. But I found that this operation, when performed but once, was by no means ſufficient for corundum. I therefore repeated it, till the ſtone appeared to be fiſſured in every direction. After this, the ſpecimen to be pulverized was put into a ſteel mortar, about three-fourths of an inch in diameter, and three inches in depth, into which a ſteel peſtle was very cloſely adjuſted. A few blows upon the peſtle cauſed the ſtone to crumble; and the fragments were then eaſily reduced into an impalpable powder, in an agate mortar, with a peſtle of the ſame material. The abraſion from the mortar, uſual in the pulverization of hard ſtones, was much diminiſhed by the above precaution; rubies and ſapphires being, in a ſhort time, ground to a powder nearly as minute as the fineſt precipitate.

Pulverized after repeated ignition and quenching.

Efficacy of the proceſs.

The great difficulty of fuſing corundum with alkali, renders that proceſs objectionable.

M. Klaproth in his analyſis before-mentioned, had obſerved with how much difficulty the ſtones were acted upon by potaſh or ſoda. I found that the greateſt heat a ſilver crucible could ſupport, without melting, was not ſufficient to produce a ſatisfactory fuſion of one part of corundum, with ſix parts of either of thoſe alkalis; nor did an expoſure to that temperature during ſeveral hours, ſeem to render the treatment more effectual. Not more than half the quantity of the corundum was ever rendered ſoluble in any acid; and what remained was the powder of the ſtone, wholly unchanged. The repeated filtrations and evaporations with which this treatment muſt be attended, not only render it tedious, but alſo produce uncertainty in the reſults. Even when very finely powdered corundum was expoſed, with ſix times its weight of potaſh, in a platina crucible, to a heat of 140° of Wedgwood, for two hours together, it was not acted upon in ſuch a manner as to be fit for analyſis. From all theſe experiments I concluded, that ſome more efficacious mode of rendering corundum ſoluble in acids was to be ſought.

I boiled a great quantity of ſulphuric acid upon very finely powdered corundum in a platina crucible. But, although the acid, after a great length of time, had diſſolved a little of the ſtone, I did not find this method more ſatisfactory than the others. Nitric, muriatic, and nitro-muriatic acids, were leſs effectual than the ſulphuric. Phoſphoric acid, held in fuſion with corundum, did not diſſolve any notable portion of that ſtone, or render it ſoluble in other acids.

Neither the volatile acids nor the phoſphoric have any notable action on corundum.

I then had recourſe to ſub-borate of ſoda (borax), which I found to anſwer beyond my expectation. Two parts of that ſalt, calcined, and one of corundum, enter into fuſion, at a temperature which I judged to be about 80° of Wedgwood *; and a glaſs, more or leſs coloured, is formed. This glaſs is ſoluble in muriatic acid; and, by this method, it is eaſy to obtain a complete ſolution of corundum. My general method of operating was as follows:

Borax fuſes it well. Two parts of calc. borax and one of cor. form a glaſs ſoluble in muriatic acid.

I took one hundred grains of corundum; and, having ſeveral times made it red-hot, and plunged it into cold water, I put it into the ſteel mortar, and treated it as already mentioned. I then poured ſome very dilute muriatic acid upon it, to waſh off whatever iron might have adhered, in conſequence of its mechanical action upon the mortar. After it was dried and weighed, I put it into the agate mortar, and ground it as fine as I could. The augmentation of weight was then noted; and was always taken into account in the general reſult. I then put the whole into a platina crucible, with 200 grains of calcined ſub-borate of ſoda, and expoſed the mixture for an hour or two to a violent heat. When the crucible was cool, muriatic acid was boiled upon it and its contents; and, in about twelve hours, all the glaſs diſappeared. If I wiſhed to obtain the ſilica directly, I evaporated the whole to dryneſs; but, if otherwiſe, I precipitated by an alkaline carbonate, and waſhed the precipitate, in order to get rid of all the ſalts contained in the liquor. This latter mode I believe to be preferable. I then re-diſſolved the precipitate in muriatic acid, and evaporated for ſilica. But, as corundum contains only a ſmall portion of this earth, there

Analyſis. 100 corundum were pulveriſed; fuſed with 200 borax; diſſolved in muriatic acid; (evap. to dryneſs for ſilica, or) precip. by alk. carbonate; abſtract the ſalts by water; rediſſolve the precip. in mur. acid; evap. for ſilica; filter the mur. ſolution; precip. by heat with exceſs of potaſh; oxide of iron is permanently depoſited, but alumina taken up by the alk.; precip. the alumina by mur. of amm. Waſh, dry, ignite, and weigh the earths.

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* I have no doubt that a lower temperature would be ſufficient.

was little or no appearance of jelly. When the ſilica was thus precipitated by evaporation, I filtered the liquor, and boiled it with an exceſs of potaſh. By this operation, the alumina was precipitated, and then re-diſſolved by the exceſs of potaſh, from which it was finally obtained by muriate of ammonia; the iron which had remained undiſſolved by the potaſh, having of courſe been previouſly ſeparated from the alumina. This earth, and the ſilica, after being waſhed and dried, were ignited, and thus the weight of both was obtained.

Example. Analyſis of the ſapphire.

I ſhall exemplify, in a ſingle inſtance, this mode of treatment; and then preſent the reſults obtained from the different kinds of corundum. For this purpoſe, I ſhall ſelect the blue perfect corundum, or ſapphire, as the ſtone which has been the moſt ably analyzed by Mr. Klaproth. From a view of both analyſes, the efficacy of the fuſion with borax will be evident; and the reſults of the ſeveral experiments may be compared.

100 grs. ſapphire were pulverized and then fuſed into a green glaſs with 250 borax,

1. 100 grains of ſapphire, pulverized in the agate mortar, as above ſtated, had increaſed to 105. Theſe 105 were mixed with 250 of calcined ſub-borate of ſoda, and put into a platina crucible. They were then expoſed to a violent heat for two hours, and afterwards allowed to cool. The maſs was vitrified, and had the appearance of a greeniſh blue glaſs, fiſſured in many directions.

which was diſſolved in boiling mur. acid.

2. This glaſs being ſtrongly attached to the platina crucible, the whole was put into muriatic acid, and boiled for ſome hours. By theſe means, a total and limpid ſolution was obtained.

Precip. by carb. of ammonia, was waſhed, dried, diſſolved in mur. and expoſ. to evap.

3. The matter of the ſtone was next precipitated, by ammonia not entirely ſaturated with carbonic acid; the liquor was filtered; and the precipitate well waſhed and dried. It was then rediſſolved in muriatic acid, and evaporated.

it gave a precip. of 10.25 ſilica.

4. By this evaporation a precipitate was formed, which, when well waſhed and ignited, weighed 10,25 grains, and was ſilica.

The water of waſhing and the mur. liquor were boiled with exceſs of potaſh. One grain of iron remained.

5. The liquor, together with that which had waſhed the precipitate, was boiled in a ſilver veſſel, with an exceſs of potaſh; this rediſſolved all the precipitate, except one grain.

Mur. of amm. was added to the alk: ſolution.

6. Muriate of ammonia was poured into the alkaline ſolution. (No. 5.) The potaſh expelled the ammonia from the muriatic acid, and, forming muriate of potaſh, could no longer retain the earth in ſolution; a very copious precipitate, therefore, was

formed. This precipitate had all the properties of alumina; and, when well waſhed and ignited, weighed 92 grains. Conſequently, deducting 5 from the ſilica, for the abraſion of the mortar, we ſhall have for reſult,

The potaſh left united with the mur. acid, and ſuffered the alumina to fall. Waſhed and ignited it weighed 92 grs.

Comp. parts deduced.

Silica - - - -	5,25
Alumina - - - -	92
Iron - - - -	1
Loſs - - - -	1,75
	100,00.

The chief difference between theſe proportions and thoſe eſtabliſhed by Mr. Klaproth, is in the ſilica. That chemiſt did not find any notable portion of it in the ſpecimens he examined. This naturally induced me to make a very ſtrict reſearch into every poſſible means by which any ſilica might have been introduced into the reſults; whether by the borax, the alkali, or any of the other re-agents I had uſed. But, finding very clearly, that none of theſe ſubſtances did contain any, I could no longer heſitate to believe, that the proportion I have here ſtated, was actually contained in the ſapphire I analyzed. I am likewiſe convinced, that no more than the quantity I have mentioned was worn from the agate mortar and peſtle; for my conſtant practice was, to weigh them, both before and after I had uſed them, in ſcales which, when charged with four pounds on each end, turn eaſily with the tenth part of a grain.

As Klaproth found little ſilica in his analyſis; this point was particularly examined.

The general reſults, from all the different kinds of corundum, were as follows:

Component parts of diff. kinds of corundum.

Blue perfect Corundum, or Sapphire.	
Silica - - -	5,25
Alumina - - -	92
Iron - - -	1
Loſs - - -	1,75
	100,00.

Red perfect Corundum, or Ruby.	
Silica - - -	7
Alumina - -	90
Iron - - -	1,2
Loſs - - -	1,8
	100,0.

Imperfect Corundum from the Carnatic.	
Silica - - -	5
Alumina - - -	91
Iron - - -	1,5
Loſs - - - -	2,5
	100,0.

Imperfect Corundum from Malabar.	
Silica - - -	7
Alumina - -	86,5
Iron - - -	4
Loſs - - -	2,5
	100,0.

Imperfect

Imperfect Corundum from China.	
Silica	5,25
Alumina	86,50
Iron	6,50
Loſs	1,75
	100,00.

Imperfect Corundum from Ava.	
Silica	6,5
Alumina	87,0
Iron	4,5
Loſs	2,0
	100,0.

Their diff. of colour ariſes from the oxidizement of the iron.

As I could not diſcover chrome, or any other colouring ſubſtance, except iron, in theſe ſtones, I can attribute their difference of colour only to the different ſtate of oxidizement of the iron; but it is impoſſible to aſcertain what that ſtate may be, from ſo ſmall a quantity.

The matrices of theſe ſtones ſubmit to the uſual analyſis.

The matrices of theſe ſtones, and the ſubſtances accompanying them, are more eaſily fuſed than the ſix kinds of corundum juſt mentioned. The uſual and well known mode of treatment by potaſh, was ſufficient to render theſe ſubſtances ſoluble in the acids. Since the many experiments of Klaproth, Vauquelin, and others, the mode of analyzing mineral bodies is become ſo familiar to chemiſts, that I ſhall mention particulars with reſpect to one only of the following ſubſtances.

MATRIX OF CORUNDUM FROM THE PENINSULA OF INDIA.

Matrix of Indian corundum. 100 grs. fuſed with potaſh; diſſolved in mur. acid; evaporated; diſſol. in ſm. exceſs of acid; left 42,5 of *ſilica.*

1. A certain quantity of this matrix was reduced to powder, in the manner already deſcribed. 100 grains of it were treated with potaſh, in a ſilver crucible: they then afforded a limpid ſolution in muriatic acid. The liquor was evaporated; and, long before the maſs was entirely dry, it had aſſumed the appearance of a jelly. When the ſaline matter in the evaporating-diſh was diſſolved in a ſlight exceſs of acid, a white powder remained at bottom, which had all the properties of ſilica, and, when waſhed and ignited, weighed 42,5 grains.

Ammonia threw down alumina and oxide.

2. Into the liquor which had ſerved to waſh the above powder, I poured ammonia. A copious precipitate was thus formed, which was ſeparated by filtration, and well waſhed.

Carb. potaſh threw down carb. lime = *lime* 15.

3. Carbonate of potaſh alſo cauſed a precipitate in the liquor of No. 2. This precipitate was found to be carbonate of lime, and weighed 23,5 grains, = 15 of lime.

The alumina and oxide were diſſ. in mur. acid; and the

4. The precipitate of No. 2. was rediſſolved in muriatic acid; then boiled with an exceſs of potaſh, and filtered. There remained undiſſolved, 3 grains, which were iron.

5. The liquor of No. 4. was precipitated by muriate of ammonia, and afforded alumina; which, being waſhed and ignited, weighed 37,5 grains.

I could alſo perceive a trace of manganeſe.

The proportions therefore are,

Silica - - -	42,5
Alumina - - -	37,5
Lime - - -	15,0
Iron - - - -	3,0
Loſs, with a trace of manganeſe -	2,0
	100,0.

alumina taken up by potaſh. 3 grs. *iron* remained.

Mur. amm. precipitated 37,5 *alumina*.

Manganeſe, a trace.

Comp. parts.

By a ſimilar treatment, the following ſubſtances, contained in this matrix, afforded the under-mentioned reſults.

Felſpar.

Silica - - -	64
Alumina - - -	24
Lime - - -	6,25
Iron - - - -	2,00
Loſs - - -	3,75
	100,00.

Component parts of felſpar;

Fibrolite.

Silica - - -	38
Alumina - - -	58,25
A trace of iron, and loſs -	3,75
	100,00.

of fibrolite;

This is the only ſtone I have ever met with, that yielded nothing but ſilica and alumina; for the quantity of iron was ſo ſmall as hardly to be taken into account. I have repeated this analyſis three times, and have not found a difference of half a grain.

Thallite in Cryſtals, with a rough Surface.

Silica - - -	45
Alumina - - - -	28
Lime - - -	15
Iron - - - -	11
Loſs - - -	1
	100.

of rough thallite;

of priſmatic thallite;

Thallite in Priſms like the Tourmalin.

Silica	40
Alumina	25
Lime	21,5
Iron	11,5
Loſs	2
	100,0.

of thallite in tranſparent fragments;

Thallite in Fragments, of a fine tranſparent Yellow Colour.

Silica	42
Alumina	25,5
Lime	16
Iron	14
Loſs	2,5
	100,0.

of fibrolite of the Chineſe corundum;

Fibrolite accompanying the Matrix of Corundum from China.

Silica	38
Alumina	46
Iron	13
Loſs	3
	100.

of felſpar from ſand of Ceylon.

Felſpar from the Sand of Ceylon.

Silica	68,5
Alumina	20,5
Lime	7
Iron	1,5
Loſs	2,5
	100,0.

Metallic crucibles alone muſt be uſed with earths;

As the greater part of the above ſubſtances were fuſible without difficulty in potaſh, I preferred uſing a ſilver crucible to any other. It may be laid down as a general rule, with reſpect to delicate experiments, that in the treatment of metallic ſubſtances, we ſhould not uſe metallic crucibles; but, in the treatment of earthy bodies, they alone are to be depended upon. The eaſily oxidizable metals cannot be employed; but ſilver and platina preſent advantages which no other metals ſeem

namely, ſilver and platina.

ſeem to poſſeſs. Theory would certainly give a general preference to platina, from its reſiſtance both to heat and to acids; and practice will juſtify this preference, in all but a ſingle inſtance. If a quantity of potaſh be kept for ſome time in fuſion, in a platina crucible, it will be found that the crucible has loſt ſeveral grains of its weight. The platina ſo diſſolved may be looked for in the potaſh; and, if this be ſaturated with muriatic acid, and evaporated, we ſhall find the well-known triple ſalt, formed by the combination of muriatic acid with potaſh and oxide of platina. This action of potaſh upon platina, does not depend upon any mechanical cauſe, ſuch as friction, the force that determines it being purely chemical. If a ſalt formed by potaſh, or a ſalt formed by ammonia, be mixed with a ſalt of platina, a precipitate enſues, which is a triple ſalt; and it is by this method, that the Spaniſh government detects the platina, in the ingots of gold ſent from their American poſſeſſions. It is therefore evident, that an affinity does exiſt between potaſh and platina, in a certain ſtate; and I imagine it to be this affinity, which cauſes the oxidizement of the platina, when potaſh is kept in fuſion upon that metal. I muſt however obſerve, that my crucible was prepared by Janetty, in Paris, according to a method he has publiſhed in the *Annales de Chimie*: and that he always employs arſenic, a little of which certainly remains united to the platina. What influence arſenic may have, remains to be determined. Soda does not form a triple ſalt with the oxide of platina; for I have frequently kept this alkali in fuſion, in a platina crucible, for a long time; yet very little action was produced upon the metal. This fact ſeems to corroborate my aſſertion, that the affinity of potaſh for oxide of platina, determines the oxidizement of the metal.

Platina is preferable; except with potaſh *which acts upon it.*

Spaniſh method of detecting platina in ingots of gold. The ſalts of platina are precipitated (triple) by the ſalts of potaſh.

Is platina affected by the arſenic it contains?

Soda ſcarcely acts upon platina.

Whenever I ſuſpected that platina had been diſſolved, I could eaſily detect the ſmalleſt portion of it. A ſolution of platina, ſo dilute as to be nearly colourleſs, manifeſts, in a very ſhort time, the colour of a much more concentrate ſolution, and becomes reddiſh, by the addition of a ſolution of tin in muriatic acid. This I have found to be, by many degrees, the moſt ſenſible teſt for platina; and it would anſwer the purpoſes of the Spaniſh government, much better than that they uſually employ.

Muriate of tin is the moſt delicate teſt of platina.

Alkalis do not act on ſilver; yet the crucibles become rather more brittle by long uſe.

The alkalis have no immediate action upon ſilver; but I have obſerved, that crucibles of this metal, after they have been a long time in uſe, become ſomewhat more brittle than they were before.

The fixed alkalis riſe by mere heat; potaſh more readily than ſoda.

Potaſh and ſoda have long been termed fixed alkalis; and it is certain that, if we compare them with ammonia, they are ſo. But *fixed* is an abſolute term, and cannot admit of degrees. If potaſh, ſuch as we obtain from Mr. Berthollet's method of preparing it, be kept in fuſion at a very ſtrong heat, it may be totally volatilized. The vapour of the alkali may be perceived in the room; and vegetable colours will undergo the change which is uſually produced by alkalis. Indeed, in preparing Mr. Berthollet's potaſh, the vapour of the alkali may be eaſily perceived. Soda is not quite ſo volatile; though far from being fixed. It appears alſo, that a little water increaſes the volatility of both potaſh and ſoda, as happens with boracic acid. This volatility of potaſh, has been advantageouſly applied of late to the art of bleaching.

Water aſſiſts their elevation.

Potaſh uſefully volatilized in bleaching.

§ II.

On the Affinities the Earths have been ſuppoſed to have for each other, in the humid way.

In the courſe of the foregoing analyſis, I had occaſion to make ſome further obſervations concerning a ſubject upon which I had been formerly engaged, namely, on the affinities the earths have been ſuppoſed to have for each other, when held in ſolution by acid or alkaline menſtrua.

On the affinities of the earths for each other.

Hiſtorical facts.

In the XXVIIIth volume of the *Annales de Chimie*, page 189, I publiſhed a paper upon the analyſis of ſome magneſian ſtones. In this paper, I took notice of the following affinities of the earths for each other, namely, the affinity of alumina for magneſia, of alumina for lime, and of alumina for ſilica. In the XXXIſt volume, page 246, there is a memoir, by Guyton de Morveau, upon a ſimilar ſubject;* and he there reports ſome experiments of his own, by which he was induced to think, that the earths do really poſſeſs a chemical attraction for one another. Since that time, the affinity of the earths has been

Guyton's experiments.

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* He has taken no notice of any of the experiments contained in my paper.

received among chemiſts as an undoubted fact; and, at the end of Mr. Kirwan's *Eſſay on the Analyſis of mineral Waters*, we find a liſt of earthy ſalts which produce a re-action upon one another, ſuppoſed to be cauſed by an affinity that tends to unite their baſes, in the form of a precipitate, inſoluble in the acids. Some other detached obſervations are to be found, in the *Journal de Phyſique*, and in the *Annales de Chimie*. The fact is certainly one of the moſt important in the docimaſtic art, and merits all the attention of the ſkilful in that branch.

Conteſted by Darracq.

In the XLth volume of the *Annales de Chimie*, page 52, Darracq has publiſhed a paper, intended as a refutation of the concluſions drawn by Guyton. I had myſelf repeated the greater part of the experiments of the latter; and the reſults I obtained were exactly ſimilar to thoſe of Darracq. In fact, I had intended to continue the reſearches; but the very ſatisfactory paper of Darracq, appeared to me to render a further proſecution of them totally uſeleſs. However, a paragraph inſerted in the *Annales de Chimie*, (Tom. XLI. p. 206.) and of which Guyton appears to be the author, ſhews that he has not derived from the Memoir of Darracq, that conviction which it certainly conveys. The paragraph in queſtion is founded upon a letter, written from Freyberg, by Dr. G. M. to Dr. Babington, dated December 17, 1800, and inſerted in the IVth volume of Nicholſon's Journal, page 511. This letter contains an opinion which deſerves to be canvaſſed, as it is not perfectly juſt; and the uſe Guyton has made of it, has determined me to add my obſervations to thoſe of Darracq.

Repetition of Guyton's experiments.

I ſhall follow the order of Guyton's experiments, in the enumeration of thoſe I made.

Lime-water and barytes-water = precipitate.—Failed.

Exp. 1. From a mixture of lime-water and barytes-water, Guyton obtained a precipitate. I obtained none.

Alumina in potaſh and ſilica in potaſh = precip.—Aſcertained before.

Exp. 2. A ſolution of alumina in potaſh, mixed with a ſolution of ſilica in the ſame, gave a precipitate, after ſtanding ſome time. This had been obſerved by Darracq, and by Guyton, and agrees perfectly with the affinity which, before Guyton publiſhed his paper, I had aſſerted to exiſt between theſe two earths.

Water of lime, or of ſtrontia, or of barytes + to ſilica in potaſh = precip.

Exp. 3, 4, 5. Lime-water, ſtrontia-water, and barytes-water, produce a ſomewhat ſimilar effect upon a ſolution of ſilica in potaſh.

Barytes water, and ſtrontia-water = no precipitate.

Exp. 6. No precipitate took place from a mixture of barytes-water and ſtrontia-water; nor from ſolutions of the carbonates of thoſe earths, in water impregnated with carbonic acid.

Mur. lime and mur. alumina = precip.—Failed.

Exp. 7. Guyton obtained a precipitate, by mixing ſolutions of muriate of lime and muriate of alumina. I could not obtain any.

Mur. lime and mur. magneſia, = no precip.

Exp. 8. Solutions of muriate of lime and muriate of magneſia, when mixed, did not afford a precipitate.

Mur. lime and mur. barytes = precip.—Failed.

Exp. 9. Muriate of barytes did not, as Guyton has aſſerted, form a precipitate with muriate of lime. He was right in ſaying, that muriate of ſtrontia gave no precipitate with muriate of lime.

Mur. mag. and mur. alum, = no precip.

Exp. 10. Muriate of magneſia and of alumina, afforded me no precipitate. Guyton ſays, that the liquors became milky.

Mur. magneſia and mur. of barytes or ſtrontia = abundant precip.—Failed.

Exp. 11. Muriate of magneſia, whether mixed with muriate of barytes or of ſtrontia, afforded me no change; although Guyton ſays he obtained an abundant precipitate, by mixing muriate of magneſia with muriate of barytes.

Mur. alum. and mur. barytes = precip.—Failed.

Exp. 12. Muriate of alumina and of barytes, did not, when mixed together, yield any precipitate. Guyton aſſerts, that there is a precipitate in this caſe.

Mur. barytes and ſtrontia = no precip.

Exp. 13. Muriate of barytes and of ſtrontia, did not form a precipitate. Guyton has remarked the ſame.

Mur. ſtrontia and alumina = no precip.

Exp. 14. From muriate of ſtrontia and of alumina, I obtained no precipitate. With Guyton the liquor became milky.

Guyton was wrong in aſſerting that barytes has affinity for lime, magneſia and alumina; and that ſtrontia attracts alumina.

From all theſe experiments it appears very clearly, that Guyton has pronounced too haſtily, upon the affinity which he ſuppoſes barytes to entertain for lime, for magneſia, and for alumina; and that he is equally in the wrong, with regard to the affinity of ſtrontia and alumina. With regard to *Exp.* 3, 4, and 5, although they appear to be true, yet it would require the reſpective precipitates to be further examined, before we admit a decided affinity between the earths. The quantity of carbonic acid alſo, which muſt of courſe combine with the potaſh, during the treatment of the ſilica by that alkali, ſhould be taken into account, in conſidering the cauſe of the precipitate.

The ſolutions which I uſed, of all the above ſalts, were in the moſt concentrate ſtate; therefore, in the ſtate moſt favourable for ſhowing precipitation, if any had taken place.

Cauſes of the errors in his e periments

It is not very difficult to account for the appearances that deceived Mr. Guyton in his experiments, and for the cauſe that produced them. In one inſtance, he obtained a precipitate from muriate of lime and of alumina, becauſe, in all probability, the alumina he diſſolved in muriatic acid had been precipitated from alum; and alumina, thus prepared, retains a ſmall portion of ſulphuric acid *. In the next place, it is very likely that his ſolutions were ſufficiently concentrate to give a precipitate of ſulphate of lime. The ſame was the caſe with regard to his mixture of muriate of ſtrontia with muriate of alumina. As to the general concluſion, that barytes has an affinity for lime, magneſia, and alumina, which ſtrontia does not appear to poſſeſs, it is to be explained as follows. Lime often contains a little ſulphate of lime. Mr. Guyton's magneſia, as well as his alumina, had probably been obtained from the ſulphate; and we are indebted to Mr. Berthollet, for the true nature of many ſimilar precipitates.

Barytes is a much more delicate teſt than ſtrontia, for ſulphuric acid; and therefore, barytic ſolutions were affected by quantities of ſulphuric acid, which ſtrontia could not render ſenſible. This I have aſcertained to be the caſe: for I have obtained copious precipitates, by barytes, in a liquor compoſed for the purpoſe, wherein ſtrontia did not produce the ſmalleſt cloud, or ſhow the preſence of ſulphuric acid.

He did not tak due care in pre paring his earths.

A little care and attention are neceſſary, in preparing the earths, which are to be diſſolved in the muriatic acid, for theſe experiments; and, if Mr. Guyton had taken the requiſite precautions, he would not have been led into error. The object to be kept in view is, to free the earth from ſulphuric acid; and, if this be obtained, there is not the ſmalleſt precipitate or cloud, in any of the caſes I have mentioned. If

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* It is ſomewhat ſingular, that Guyton ſhould have obſerved this fact elſewhere. See his experiments on the diamond, in the *Annales de Chimie*. The preparation of a barytic ſalt, by alumina prepared from the ſulphate of this earth, had been obſerved by Scheele, in his *Eſſay on the Affinities of Bodies*. But that great chemiſt referred the phenomenon to its right cauſe, viz. to ſome ſulphuric acid remaining in all alumina thus prepared.

any further proof be neceſſary, with regard to the cauſe of precipitates obtained in the manner ſtated by Mr. Guyton, I may add, that I have repeated his experiments, and have always found the precipitates to be ſulphate of barytes.

Mr. Kirwan has made ſimilar miſtakes.

The general concluſion to be drawn from the obſervations of Mr. Kirwan, already alluded to, is, that barytes has an affinity for lime, magneſia, and alumina, upon which earths ſtrontia does not ſeem to have any influence. But theſe miſtakes are to be accounted for in the ſame manner as thoſe of Mr. Guyton, viz. by ſulphate of barytes being much leſs ſoluble than ſulphate of ſtrontia, and therefore ſhowing the preſence of a ſmaller portion of ſulphuric acid, or, in other words, being a much more delicate teſt for that ſubſtance.

Letter in Nicholſon's Journal,

With regard to the letter already mentioned as being inſerted in Nicholſon's Journal, and which drew ſome reflections from Mr. Guyton, it is neceſſary to examine as much of it as may be thought objectionable.

aſſerting that becauſe a precipitate of ſilica is ſoluble in acids, the reſults of analyſis with alkalis muſt be fallacious.

The author ſays, that he repeated the experiments of Mr. Guyton, with an alkaline ſolution of ſilica and alumina, and that he obtained a precipitate; which precipitate, though containing ſilica, was totally ſoluble in the acids. "Here," he ſays, "the properties of the ſilex muſt be conſiderably altered. This muſt render all analyſis with alkalis ſuſpicious; and ſhews on what fallacious grounds the proud dominion of chemiſtry reſts, which ſhe has exerciſed ſo long, in ſuch an arbitrary and overbearing manner, in the mineral kingdom." This opinion is by no means likely to overthrow the pretenſions of chemiſtry; for the very circumſtance of rendering ſilica ſoluble in the acids, is one of the diſcoveries that has moſt contributed to render certain, and to extend, our knowledge of analyſis. No earthy ſubſtance is now thought fit to be ſubmitted to further experiment, till a complete ſolution of it in an acid be firſt obtained; and, when that ſolution cannot be effected directly by the acid, it is always attempted by previous fuſion with an alkali. This mode of rendering ſilica ſoluble in acids, is no new diſcovery; it has been long known; and the analyſis of minerals has never been brought ſo near to truth, as ſince it has become an indiſpenſable condition.

This fact is on the contrary the greateſt improvement in modern analyſis.

Alumina attracts ſilica;

I have no doubt as to the fact of a precipitate being formed, by mixing together an alkaline ſolution of ſilica and alumina. Alumina indeed appears to exerciſe an attraction, as I before ſtated, for ſilica, for magneſia, and for lime. All ſtones in which there is but little alumina, and a great quantity of ſilica, leave, after fuſion with potaſh, a light and flocculent ſubſtance, which cannot be diſſolved by the acids: this ſubſtance, however, which is ſilica, has been in ſolution in the alkali. But, if a greater proportion of alumina be preſent, none of this flocculent precipitate appears; hence it is evident, that alumina muſt determine its ſolution. Its eaſy ſolubility, in the latter caſe, cannot depend upon the diviſion of the particles of the ſilica in the ſtone; for, in the firſt place, after being fuſed with potaſh, the tenuity of the particles of every ſtone muſt be nearly the ſame; and, in the next place, I have not obſerved, that any earth, except alumina, can promote the chemical ſolution of the ſilica, though they muſt all occaſion its mechanical diviſion.

and promotes its ſolubility.

As to the affinity of alumina for magneſia, it is by much the moſt powerful of all thoſe which any of the earths have for each other. I attempted to precipitate magneſia from muriatic acid, by ammonia, even in exceſs; but found that the whole muriate of magneſia had not been decompoſed, and that a triple ſalt, or an ammoniacal muriate of magneſia *, had been formed. I then poured an exceſs of ammonia into a ſolution of muriate of magneſia, mixed with a large proportion of a ſolution of muriate of alumina. All the earth was precipitated; and nothing remained in ſolution, except muriate of ammonia. The liquor was then filtered, and the precipitate waſhed and dried. I diſſolved it in muriatic acid, and boiled it with a great exceſs of potaſh. Some alumina was taken up, but by no means all the quantity that had been uſed. The precipitate which had reſiſted the action of potaſh, was again diſſolved in muriatic acid, and precipitated by carbonate of potaſh. The carbonate of magneſia was held in ſolution by the exceſs of carbonic acid; and, by uſing potaſh and carbonic acid alternately, (the firſt to diſſolve alumina, the ſecond to diſſolve carbonate of magneſia,) I effected a ſeparation of the earths. Theſe experiments ſhew, that there is an affinity between alumina and magneſia, and a certain point of ſaturation, where the action of potaſh upon alumina is wholly counteracted by the affinity of that earth for magneſia.

Alumina very powerfully attracts magneſia.

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* This ſalt is well known in chemiſtry.

Alumina attracts lime; which becomes diſſolved in potaſh along with it.

When a ſolution of potaſh is boiled upon a mixture of lime and alumina, the alumina is diſſolved, together with a much greater portion of lime than can be attributed to the diſſolving power of the water alone. But, if a ſolution of potaſh be boiled upon lime, without alumina, no more lime is taken up than would have been diſſolved by an equal quantity of water not containing potaſh in ſolution; conſequently, alumina ſeems really to promote the ſolution of lime in potaſh. The affinity of alumina for lime, I had mentioned in the paper to which I allude; and it has ſince been noticed by Mr. Vauquelin*.

Guyton's concluſions, if true, would have rendered analyſis uncertain.

Objection to Berthollet's unlimited poſition reſpecting attraction from the maſs.

If the concluſions of Mr. Guyton had been well founded, it would have been chemically impoſſible to arrive at truth in analyſis. There were already real difficulties enough to be overcome; and Mr. Berthollet has lately diſcovered ſome, which are not ſo eaſily anſwered as thoſe I have juſt conſidered. The poſition of this chemiſt, however, has been too generally extended by him. If the power of maſſes were as great as he repreſents it to be, and if it increaſed *ad infinitum*, in proportion to the maſs, it muſt follow, that, with any given ſubſtance, we could decompoſe any compound, provided the maſs of the decompounding body were ſufficiently great; but this is well known not to be the caſe.

From the experiments which I have related, it appears to be proved,

Recapitulation of the affinities of the earths, &c.

1ſt. That there exiſts an affinity between ſilica and alumina.

2dly. That there exiſts a very powerful affinity between alumina and magneſia.

3dly. That alumina ſhews an affinity for lime; but that the ſaid affinity is not ſo ſtrong as Mr. Guyton had ſuppoſed, nor, if pure reagents be uſed, is it to be perceived under the circumſtances ſtated by him.

4thly. That Mr. Guyton was miſtaken in every inſtance of affinity between the earths, excepting in the caſe of ſilica with alumina, which had been obſerved before his experiments; and that, in the other caſes, he has attributed to a cauſe which does not exiſt, phenomena that muſt have reſulted from the impurity of his reagents.

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* Scheele was, in fact, the firſt who perceived this affinity. See his *Eſſay on Silex, Clay, and Alumina.*

5thly. That neither the experiments of Mr. Guyton, nor the opinion maintained in the letter from Freyberg, are ſufficient to diminiſh, in any degree, the value of the aſſiſtance mineralogy derives from chemical inveſtigation.

IV.

A Memoir on the Muſical Sounds produced in Tubes by Hidrogen Gas. Read to the Society of Philoſophy and Natural Hiſtory at Geneva. By G. DELARIVE *.

IN a former meeting our learned colleague, Profeſſor Pictet, communicated to the Society a ſeries of experiments on the *muſical ſounds produced in tubes by hidrogen gas,* in which he developed the various muſical phenomena which theſe tubes produce. He explained the effect which the length or width of the tube, and the ſituation where the hidrogen is burned, have on the ſounds produced. As to the cauſe of the ſound, he gave only a few conjectures; his inquiries not being directed to that object. The purpoſe of the preſent memoir is to diſcover this cauſe.

Profeſſor Pictet's reſearches on muſical tubes by hidrogen.

Profeſſor Brugnatelli is, I think, the firſt who publiſhed the experiment I have endeavoured to explain †: It was invented by a German. I ſhall here relate the principal circumſtances.

Publication by Brugnatelli.

If a current of inflamed hidrogen gas be incloſed in a tube of an elaſtic and ſonorous material, ſuch as glaſs, metal, dry wood, &c. the tube after an interval of ſome ſeconds, will produce a muſical ſound. If it is open at both ends, the ſound will be ſtrong and full. It is poſſible however to ſucceed with a tube hermetically cloſed at one end, provided its diameter be large enough to allow of a circulation of atmoſpheric air ſufficient to keep the gas in a ſtate of combuſtion. The conditions eſſential to the ſucceſs of the experiment are, *firſt,* That the tube be elaſtic and capable of forming an echo, that is to ſay, capable of reflecting the undulations which proceed from the ſonorous point; for no ſound will be produced with a tube of paſteboard or paper: and, *ſecondly,* The flame muſt be produced by

Deſcription of the manner of burning hidrogen in a tube, to produce a muſical ſound.

The tube muſt be elaſtic;

and the flame from hidrogen.

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[*] Journal de Phyſique, LV. 165.

[†] Dr. Higgins diſcovered it. See Phil. Journal, new ſeries, I.

a current of hidrogen; for an inflamed ſtream of the vapours of alcohol or ether, a lighted taper, &c. are incapable of producing any ſound.

Circumſtances.

The place of combuſtion is the ſonorous point.

Undulations at the point muſt accord with the reſonaut echo of the tube.

The ſound does not iſſue at firſt.

Its tone.

Let us now examine the circumſtances of the experiment. There muſt be a point which may be called the *ſonorous point;* at which the vibrations, communicating an undulatory motion to the air, are produced. This point is the place of combuſtion; for if this be made to alter its ſituation, the ſounds will vary, as M. Pictet has proved in a ſeries of experiments. He has likewiſe obſerved, by means of ſmoke with which he filled the tube, a continual ſucceſſion of vibrations at this point; theſe vibrations produce the waves, which proceeding with a determinate velocity, ſtrike againſt the ſides of the tube, and are reflected with a velocity equal to that with which they arrived. When the diſtance of the ſides of the tube is ſuch that the alternate reflections are equal to the vibrations natural to the ſonorous cauſe, the ſound increaſes in intenſity, and becomes muſically appreciable. It appears likewiſe that the waves thus reflected do re-act on the primitive vibrations produced at the place of combuſtion, ſo as to render them harmoniouſly regular. For a certain ſpace of time is almoſt always neceſſary before the inſtrument acquires a regular and full ſound; and the tone or pitch of the tube will be more or leſs acute according to the greater or leſs number of undulations which take place in a given time.

The temperature of the air in the tube is hotteſt at the burning point, and cooler more remote.

Alcohol or ether give a leſs degree of heat.

There is another fact eſſential to be obſerved in this experiment. The temperature of the column of air is not the ſame through all its length. At the ſonorous point, that is to ſay the place of combuſtion, the temperature is very high; ſo much ſo that the end of the adjutage of the glaſs through which the hidrogen paſſes, is conſtantly in a ſtate of ignition. If an inflamed ſtream of the vapours of alcohol or ether be ſubſtituted inſtead of the hidrogen, the heat is evidently leſs. From ſome experiments it alſo ſeems probable that the temperature of the room, and the purity of its air, may affect the ſucceſs of the experiment.

Conjecture that the ſound is produced by the rapid production and collapſion of aqueous vapour.

My experiments have been directed to aſcertain the cauſe of theſe phenomena, how and by what means theſe ſonorous vibrations are produced. We know that water is formed during the combuſtion of hidrogen: this water appears in the form of vapour. The temperature of the place of combuſtion being very elevated, theſe vapours muſt occupy an extenſive ſpace; but by immediately coming into contact with a cold air, their volume muſt rapidly diminiſh. A vacuum muſt thus be formed into which the air muſt collapſe, and be again driven by other vapours, which again contract in their turn. Do not the vibrations reſult from theſe alternate motions, produced by the great expanſion and ſubſequent contraction of the vapours [*]? Such were the conjectures that might be formed as to the probable cauſe of the phenomenon; but accident has preſented me a fact which appears to give them ſome weight.

Sound produced by heating water in a thermometer bulb and tube.

I had a thermometer tube of about one line in diameter, at the extremity of which was a ſmall bulb: there was a drop of water which I wiſhed to expel from it; for this purpoſe I repeatedly expoſed the bulb to the flame of a lamp with alcohol, and was agreeably ſurpriſed to hear a muſical ſound proceed from the tube.

Inſtructions for making the experiment.

In order that this experiment may ſucceed, a tube of one, two, or three lines in diameter, ſhould be taken, of three, four, or five inches in length, with a bulb at one of the extremities, in diameter about three times that of the tube; it need not be very regular. If it were even a little flattened, I think the ſound would be louder. A very ſmall quantity of water or mercury muſt then be introduced, and expoſe it to a ſtrong

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The ſound of air falling into a vacuum much more intenſe than that from exploſion.

[*] It appears probable to me, that the ſound produced by air precipitating itſelf into a vacuum, is more intenſe than that reſulting from an expanſive force. The horrible noiſe occaſioned by the detonation of ſoap bubbles of hidrogen and oxigen is well known, though the lighteſt objects ſurrounding the baſon are not even ſhaken. Whence it may be concluded, that the phenomenon is produced by the ſudden vacuum proceeding from the deſtruction of the gaſes. The detonation of a piſtol with inflammable gas is much greater than that of an air-gun, though the effect is leſs conſiderable; probably becauſe a vacuum in the piſtol ſucceeds the firſt expanſive force. The child's toy, called the *humming top*, is well known. It is a hollow ſphere, with an opening at its circumference: it produces a very ſtrong humming noiſe when ſpinned rapidly on its axis. What may be the cauſe of this humming? I think it is the ſame as that which I have juſt mentioned; the centrifugal force drives the air from the ſphere through the opening; a kind of vacuum is made, into which the external air continually preſſes, and is conſtantly driven back; whence proceed a ſeries of ſonorous oſcillations.—D.

The humming top.

heat; that of a lamp with alcohol is commonly ſufficient; but the flame muſt be large and ſtrong when the tube is large. After the tube has been thus expoſed for a few ſeconds, the ſound is heard. Thoſe tubes whoſe diameters are conſiderable give a deep ſound, and the ſize of the bulb likewiſe ſeems alſo to contribute to the ſame effect: the ſound continues for ſeveral ſeconds, and then gradually decays, and at length entirely ceaſes. By ſuffering the apparatus to cool, and cauſing the condenſed liquid to deſcend into the bulb, the experiment may be repeated as often as ſhall be thought neceſſary.

Inveſtigation of both kinds of ſound.

Such is the experiment by means of which I think the phenomenon of the muſical tubes may be ſatisfactorily explained. Let us firſt examine what paſſes in the tubes with bulbs, with the eſſential conditions under which they produce their ſound, and then endeavour to diſcover the cauſe. I will then compare it with the ſound produced in tubes with hidrogen, and enquire in what reſpects the effects produced by theſe two inſtruments reſemble one another, with the differences they preſent, and the cauſe of thoſe differences.

Conditions for the tube and bulb.

There muſt be a bulb;

containing water;

or rather mercury;

but not ether, alcohol, or ſulph. acid.

The fluid muſt be very ſmall in quantity.

The heat muſt be ſtrong, to the bulb;

and the tube kept cold.

There muſt be ſome air in the tube.

The conditions eſſential to the production of ſound in tubes with bulbs are, *1ſt*, That the tube ſhould have a bulb. I never could excite ſonorous vibrations in a tube ſimply cloſed at one end. *2d*, This bulb muſt contain an evapourable liquid. Water ſucceeds very well, but it has this diſadvantage; it forms, in paſſing from the vapourous to the liquid ſtate, a drop of water in the tube, which often completely obſtructs it; and at other times, by running along the warm part of the glaſs, it frequently breaks it. Mercury has not this inconvenience: I never could ſucceed in producing ſounds with ether, alcohol, or concentrated ſulphuric acid. The ſounds are influenced by the quantity of liquid contained in the bulb. It ſhould be as ſmall as poſſible. If there be too much, the vapour fills the tube, and by completely expelling the air, renders it equally warm throughout, and no ſound can then be produced. *3d*, The third condition eſſential to the ſucceſs of this experiment, is the application of a ſtrong heat to the bulb while the reſt of the tube continues cold; for if there be not a marked difference in the temperatures of the tube and of the bulb, no ſonorous effect will take place. *4th*, The preſence of atmoſpheric air in the tube is indiſpenſable; if it be entirely excluded, no effect will be produced; and in every ſtage of the experiment it will be found that the vapour occupies only a certain part of the inſtrument, and that air is always preſent. I have frequently endeavoured to determine the exact ſpace occupied by the vapour at the moment the ſound was heard, and I found, at leaſt in the ſmall tubes, that it was nearly equal to the volume of the bulb. In order to determine this, I cloſed the orifice of the tube with my finger at the moment it began to ſound; I then plunged the end of the tube in mercury, withdrew my finger, and ſuffered the apparatus to cool. The vapour thus became condenſed, and the ſpace it occupied was eſtimated by the quantity of mercury which the preſſure of the atmoſphere cauſed to riſe in the tube.

Proof of this.

Recapitulation.

Such are the four conditions eſſential to the production of ſounds; namely, a bulb at the extremity of the tube; the preſence of a very ſmall quantity of water or mercury in this bulb; the application of a ſtrong heat to it, whilſt the remainder of the tube remains cold; and, laſtly, the ſimultaneous preſence of atmoſpheric air and vapour in the apparatus; it is ſcarcely neceſſary to add, that the orifice of the tube ſhould be always open. Let us now conſider what may be the cauſe of the ſound. I firſt endeavoured to determine whether any chemical decompoſition of the liquid employed took place. For this purpoſe I uſed a tube ſufficiently long to permit all the liquid to condenſe in it; I weighed it carefully before I ſubjected it to experiment, and found that its weight was neither augmented nor diminiſhed after repeated productions of ſound: Whence I conclude, that the caloric has no chemical effect on the liquid, but that it only experiences ſucceſſive evaporations and condenſations. Muſt we then attribute the ſound to the evaporation and condenſation of the liquid? I thought ſo at firſt, but the following conſiderations made me alter my opinion: I firſt obſerved that it was poſſible to have a ſucceſſive evaporation and condenſation of the liquid, without producing ſound: this was effected by applying heat not ſufficiently intenſe to produce ſound. Secondly, When the experiment was made with a drop of water, I always found that the moment when the apparatus began to ſound, was that at which the whole fluid had evaporated, and conſequently that the caloric acts on the vapour alone. While the ſmalleſt atom of liquid water remained in the bulb, no ſound was produced. From this fact I have deduced, that the ſound is produced by the action

The ſound is not produced by chemical change;

nor by evaporation and condenſation;

for theſe laſt may happen without ſound;

but to a tremulous expanſion and contraction of the vapour; which thus ſtrikes the air and produces ſound.

It ſeems as if the expanſions of the vapour ought to be iſochronous with the pitch or action of the tube governed by its length and diameter.

Theſe ſounds ceaſe as ſoon as the tube has become too hot to occaſion the ſudden contraction of the vapour.

Experiments in confirmation.

action of caloric on the vapour and its re-action on the atmoſpheric air. I conceive that this phenomenon takes place in the following manner: The vapour contained in the bulb, by means of an addition of caloric applied on all ſides and in great quantity, acquires an augmentation of volume and elaſticity; it rapidly paſſes from the bulb into the tube, and drives out the air contained in it; but this air and the ſides of the tube cauſe it again to loſe a portion of its caloric, and its volume immediately diminiſhes. A vacuum is thus made, and the air reſumes its primitive ſituation. Another addition of caloric reſtores its elaſticity, which it ſpeedily loſes in the ſame manner. A ſeries of oſcillations by this proceſs give the air an undulating motion; and the waves being reflected by the ſides of the tube, become ſonorous and appreciable as ſoon as their progreſs and receſs are iſochronous with the oſcillations produced by the cauſe here pointed out. There are ſome tubes which never produce any ſound: In this caſe I preſume that the advance and receſs of the waves cannot harmonize with the primitive oſcillations, but that the one deſtroys the other. After a certain time the ſound diminiſhes and dies away: This is explained by the propagation of the heat along the ſides of the tube when the bulb is very warm and the tube cold. The vapour when firſt driven from the bulb, *ſuddenly* loſes a part of its volume, and the oſcillations thus produced are ſtrong and frequent; but when the tube has acquired a certain degree of heat, the vapour then *gradually* diminiſhes in volume as it paſſes from a very high temperature to a ſituation certainly leſs warm, though of a ſufficient degree of heat to cauſe the oſcillations to become weaker, and at length entirely to ceaſe. That this is the cauſe of the ceſſation of the ſound, may be ſhewn by applying a ſtrong heat to the part of the tube already warmed, and at the ſame time keeping up the firſt degree of heat at the bulb. By this means the limit of temperature is again preciſely determined, and the ſound reproduced in all its force. It may eaſily be conceived that the ſubſtance of the tube ought to be a non-conductor of heat, and on this account glaſs is preferable to any other ſubſtance.

Concerning the ſounds produced by hidrogen. Repetition of the theory already ſtated.

Let us now compare the tubes which ſound by means of hidrogen with thoſe with bulbs. In the former we have every condition neceſſary to the production of ſound; a very hot vapour, and conſequently very elaſtic; for as we obſerved, the

Expanſions and collapſions of aqueous vapour near the point of combuſtion.

the period of combuſtion is at ſo high a temperature, that it always ignites the tip of the glaſs tube: this hot elaſtic vapour, at the moment of its production, comes in contact with the cold air, which enters through the bottom of the tube and goes out at the top. Its volume muſt be diminiſhed the inſtant it ſtrikes this air; other heated vapours ſucceed the former, and are alſo contracted: this alternate expanſion and contraction produces the undulatory motion of the air, and cauſes the ſonorous waves.

Alcohol and ether burn with leſs heat, and produce elaſtic fluid which is neither rapidly nor totally condenſed.

Why there is leſs heat.

We have found that an inflamed ſtream of alcohol or ether produced no ſound in a tube. This is a proof of my obſervation, that in order to produce ſound, there muſt be a great difference in the temperature of the vapour and the ambient air. There is certainly, in this caſe, a formation of vapours and ſucceſſive condenſations, for the water runs along the ſides of the tube; but the point of combuſtion is infinitely leſs hot than that of hidrogen, conſequently the vapour produced is much leſs heated and elaſtic. This caſe is ſimilar to that formerly mentioned, where it was ſhewn that vaporization and ſucceſſive condenſation of the liquid might be effected without ſonorous undulation, merely by expoſing the bulb to a certain degree of heat not intenſe enough for this laſt purpoſe. We cannot be ſurpriſed that there is leſs heat produced by the combuſtion of alcohol or ether than by that of hidrogen, if we conſider, that in the latter caſe all the caloric contained in the gas and the atmoſpheric oxigen which is conſumed, becomes ſenſible heat, and is paſſed totally into the vapour produced. On the contrary, in the combuſtion of an inflammable ſubſtance, ſuch as alcohol, the caloric of the oxigen conſumed alone becomes ſenſible, and this alſo is moſtly abſorbed by the production of carbonic acid gas; ſo that the exceſs alone paſſes into the vapour. It is not therefore ſurpriſing that the heat is not ſufficient to give the vapour elaſticity ſufficient to produce ſounds. The preſence of carbonic acid gas, which is one of the reſults of the combuſtion, may alſo prevent the ſonorous vibrations.

The ſound by hidrogen is ſtrong and permanent, becauſe the conſtant ſupply of cold air produces the condenſations inceſſantly.

The ſound is much ſtronger in the tubes with hidrogen, than in thoſe with bulbs. It is likewiſe more permanent; for the tube being open at both ends, a current of cold air conſtantly enters at the bottom and iſſues out at the top: this current of air takes up and carries off the hot elaſtic vapours, receives their impulſe, and by attracting a portion of their caloric, diminiſhes their volume. This then is the moſt eſſential condition to the production of an intenſe permanent ſound; namely, a great difference between the temperature of the air and the vapour; and this difference remains always the ſame, on account of the continual renewing of the air. The ſame effect does not take place in the tubes with bulbs, and accordingly their ſound is weaker and leſs durable.

Not ſo in the bulbs.

The ſounds by hidrogen are leſs intenſe in a warm room filled with company; becauſe the air is warmer, and the combuſtion weaker for want of oxigen.

From the knowledge of this condition, that a great difference between the temperature of the air and the vapour is neceſſary to the production of ſound, it will be eaſy to comprehend that every circumſtance which augments the heat of the current of air, and diminiſhes that produced by the combuſtion of the gas, will tend to weaken and even to annihilate the ſound of the tube. Theſe two circumſtances are both found in a warm room filled with company. The current of air inſtead of being cold is heated, and the quantity of oxigen being leſs, the heat of the combuſtion is leſs. It is not then ſurpriſing that in ſuch rooms the experiment does not always ſucceed.

Sounds by phoſphorus.

Brugnatelli produced ſounds by the ſimple combuſtion of phoſphorus in tubes. Some philoſophers being perſuaded that ſonorous effects could only be attributed to hidrogen, were diſpoſed to infer its preſence in phoſphorus. From what we have ſtated, it ſeems more ſimple to explain the phenomenon by the production of phoſphoric acid in the vaporous form, which becomes ſtrongly elaſtic by means of the caloric diſengaged during the combuſtion, but of which the volume diminiſhes by the contact of cold air. We have here the alternate expanſion and contraction neceſſary to the production of ſound.

Concluſion.

Such are the ſmall number of obſervations which I have had an opportunity to make on the ſonorous tubes. I hope that they will intereſt ſuch as are more particularly engaged in this branch of natural philoſophy, and tend to direct their attention to a curious fact which has not hitherto been ſufficiently examined.

ANNOTATION.—W. N.

That aqueous vapour is condenſed with great rapidity;

AS the fundamental effect in this very ingenious explanation (namely, that aqueous vapour is condenſed with extreme rapidity), may not appear to ſome readers to be ſo ſpeedy as to produce ſonorous undulation, I think it will not be impertinent to mention a few facts relating to that ſubject. In the ſmall glaſs inſtrument called the water-hammer, which is a tube of about three quarters of an inch in diameter, with a ball of about an inch and half at one end, the other end being hermetically cloſed; the ball contains water, and the empty ſpace is rendered nearly vacuous by boiling the fluid previous to ſealing it. In this inſtrument the heat of the hand, applied to the wetted tube, is ſufficient to produce bubbles of vapour which enter the ball, but ſpeedily collapſe. The ſeries of theſe condenſations is as quick as fifteen or ſixteen in a ſecond. But in the ſteam engine the condenſation is prodigiouſly more rapid. I have a ſmall double ſteam engine on the conſtruction of Boulton and Watt, having all the parts and gear of the large engines, but its cylinder is only 2½ inches diameter, and the length of ſtroke 6¼ inches. When this was ſet to work the other day, in a lecture to my pupils, it gave 600* ſtrokes per minute. By an eaſy calculation it will be ſhewn, that the ſteam condenſed was then much more than 300 cubic inches per ſecond; and if the condenſation, inſtead of being effected in maſſes of about a pint at a time, could have been performed by ſucceſſive collapſions of each cubic inch in an open ſpace, the pulſes would have produced the tone of the loweſt E flat in the treble cliff. But the number of cubic inches condenſed in a large ſteam engine, for example, a three feet cylinder with an eight feet ſtroke, will be eight or nine times as much, at the uſual rate of working.

ſhewn by the water-hammer,

and by the ſteam engine.

A ſmall ſteam engine working twice as quick as the balance of a watch; and condenſing ten pints of ſteam in a ſecond.

This condenſation, if by ſucceſſive inches, would give a very loud E flat.

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* About twice as many as the beats of a common watch.

V. Account

V.

Account of a ſimple Method of eſtimating the Changes of Volume produced in Gaſes, by Alterations of Temperature, and of Atmoſpheric Preſſure, in the courſe of Chemical Experiments. By MR. H. DAVY. *Profeſſor of Chemiſtry at the Royal Inſtitution. From their Journal.*

Elaſtic fluids affected by preſſure and temperature.

IT often happens that changes of atmoſpherical preſſure, and of temperature, take place in the courſe of experiments on elaſtic fluids; and a knowledge of the alterations they produce in their volume, is eſſential to the preciſion and accuracy of the reſults.

Quantities of gaſes muſt be eſtimated at ſome ſtandard.

In caſes when chemical changes are produced, it is impoſſible to gain this knowledge by direct obſervation; and, in conſidering quanties, it is always uſeful to eſtimate the volumes of gaſes at ſome ſtandard, fixed upon in meaſures of the barometer* and thermometer.

Law for preſſure. Volume, inverſely as preſſure.

It is demonſtrated by very accurate experiments, that the volumes of elaſtic fluids are inverſely as the weights compreſſing them. And, conſequently, the changes produced upon gaſes, by known changes in the atmoſpherical preſſure, may be aſcertained in a very eaſy manner. With regard to the effects of temperature, however, it is much more difficult to form a juſt eſtimation by means of general laws. For though the excellent experiments of Mr. Dalton †, and thoſe of Mr. Gay Luſſac, ſhow that all the different elaſtic fluids, taken at equal temperatures, expand equally by heat, yet we are ſtill ignorant of the preciſe expanſion for increments of ſingle degrees, or of the mode in which the power of expanſion is affected by difference of preſſure; or by its relations to aqueous vapour or uncombined moiſture.

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* Lavoiſier's Elements, p. 406, 2nd Edit.

† Mancheſter Memoirs, Vol. V. p. 599. Mr. Dalton ſays, "I have repeatedly found that 1000 parts of common air, of the temperature of 55° and common preſſure, expand to 1321 parts in the manometer; to which adding 4 parts for the correſponding expanſion of glaſs, we have 325 parts increaſe upon 1000 from 55° to 212°; or for 157° of the thermometric ſcale. As for the expanſion in the intermediate degrees, which Col. Roi's experiments ſhow to be a *ſlowly diminiſhing* one above the temperature of 57°, but which de Morveau's, on the contrary, ſhow to be a *rapidly increaſing* one in the higher part of the ſcale; I am obliged to allow that Col. Roi is right, though it makes, in ſome degree, againſt an hypotheſis I have formed relative to the ſubject; he has certainly, however, made the diminution too great from 72° downwards, owing

Calculation may be avoided by comparative obſervations,

The calculations on this ſubject are conſequently laborious and complicated; but it is very eaſy to avoid them, by recurring to comparative obſervations, which may be obtained in a very ſimple manner, by means of the manometer.

with a ſtandard quantity of common air,

By making a given quantity of air, in contact with water at a known temperature and preſſure, a ſtandard, it is eaſy, in aſcertaining the changes produced in it by alteration in the temperature and preſſure, to determine, by the rule of proportion, the changes that have been produced from the ſame cauſes in any other quantity of gas, ſubmitted to chemical operation; or to aſcertain what would be its volume at the mean height of the barometer and thermometer.

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owing to his not perceiving that he actually deſtroyed a portion of the elaſtic fluid he was operating upon (aqueous vapour) in reducing its temperature ſo low; if his air had been previouſly dried by ſulphuric acid, &c. he would not have found ſo remarkable diminution below 72°. My experiments give for 77°½ above 55°, 167 parts; for the next 77°½ only 158 parts: and the expanſion in every part of the ſcale ſeems to be a gradually diminiſhing one in aſcending.

" The reſults of ſeveral experiments made upon hydrogenous gas, oxygenous gas, carbonic acid gas, and nitrous gas, which were all the kinds I tried, agreed with thoſe on common air, not only in the total expanſion, but in the gradual diminution of it in aſcending: the ſmall differences obſerved never exceeded 6 or 8 parts on the whole 325: and differences to this amount will take place in common air, when not freed from aqueous vapour, which was the ſituation of all my factitious gaſes.

" Upon the whole therefore I ſee no ſufficient reaſon why we may not conclude, that *all elaſtic fluids under the ſame preſſure expand equally by heat*—and that *for any given expanſion of mercury, the correſponding expanſion of air is proportionally ſomething leſs, the higher the temperature.*"

Mr. Gay Luſſac makes the dilatation to be from 100 to 137.5 between 32° and 212°, which gives for each degree between theſe points $\frac{1}{493}$. See p. 134; and Annales de Chimie, No. 128, p. 137.

confined in a recurved tube by water in the bend;

graduated

the water kept level, by a plunger.

In forming the manometer for the purpoſe of compariſon, a glaſs tube may be uſed about ſixteen inches long, and one third of an inch in diameter, cloſed at one end, and curved in ſuch a manner that its open leg is parallel to its cloſed end, and nearly three inches long. Its capacity is determined, and its cloſed leg is graduated ſo as to form a ſcale of 200 parts. The ſtandard volume of air is confined in it by a column of water, four inches long; and the height of this water is kept equal in both legs at the different times of obſervation, by means of a glaſs tube, moveable in a perforated cork inſerted in the open end, and capable of elevating the column of water in it at leaſt an inch and a half.

Uſe of the ſtandard of compariſon.

Inſtance.

In employing the ſtandard of compariſon, for correcting the reſults of operations in which portions of elaſtic fluids are either abſorbed or generated, it is only neceſſary to recur to the ſtate of it at the beginning and end of the experiment. Thus, let n equal the quantity of elaſtic fluid exiſting after the experiment; m the volume of the ſtandard air before the experiment; and v the volume after, as expreſſed in the ſcale of 200 parts.

Then $\frac{nm}{v} = x$, which is the volume the reſidual gas examined would occupy at a temperature and preſſure ſuch as exiſted at the commencement of the experiment.

By the ſame method may be eſtimated the volume a quantity would occupy at ſuch temperatures and preſſures, as had been at any time denoted upon the ſcale.

It is moſt probable that the reſults are not diſturbed by the aqueous vapour.

From the lateſt experiments that have been made [*], it is probable that at the ſame temperatures, and under the ſame preſſures, equal volumes of the different elaſtic fluids, in contact with water, contain the ſame quantity of aqueous vapour; ſo that in caſes when the gaſes examined in the comparative obſervations are equally ſaturated with water, the reſults muſt be perfectly accurate as to the relation of volume to the ſtate of moiſture; and, even ſuppoſing a difference in the degree of ſaturation, the error ariſing from this circumſtance, at common temperatures, would be ſo ſmall as to be inappreciable.

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[*] Thoſe of Sauſſure and De Luc, and MM. Deſormes and Clement.

VI.

Obſervations on Sir George Shuckburgh Evelyn's Paper in the Philoſophical Tranſactions for* 1798, *on the Standard of Weight and Meaſure. By* J. FLETCHER, *Eſq. Communicated by the Author.*

Occaſion of this eſſay.

Numerical errors in Sir G. S. Evelyn's paper.

Apparent difference in reſults.

Miſtake in the ſolid content of his cylinder.

1. HAVING about two years ago had occaſion, in the courſe of ſome inveſtigations in which I was then engaged, to aſcertain with as much preciſion as was poſſible the relation between the known meaſures of diſtilled water and their reſpective weights at different temperatures; Sir George Shuckburgh Evelyn's Paper on this ſubject was of courſe conſidered as the beſt authority with regard to what had been previouſly done in this reſpect. I found however, on looking into it, ſome errors in the calculations, which render the deductions from them incorrect, at the ſame time that they more ſtrongly confirm and eſtabliſh the accuracy of the experiments on which they are founded. I have no doubt, therefore, that the gentleman to whom we are ſo highly indebted for the communication of the facts which he ſtates in this important eſſay, will feel rather obliged than otherwiſe by their correction. The circumſtance which firſt induced me to repeat the calculations, was the difference which there appears to be in the reſults of the experiments with the cube and ſphere, and that with the cylinder, and which Sir George aſcribes, in § 24 and 31 of his Paper, to either a difference in the denſity of the water at different depths, or to the compreſſibility of the cylinder employed. I was however as much pleaſed as ſurprized to find, that it altogether ariſes from a miſtake in the multiplication of the four numbers whoſe product is equal to its ſolid content, *viz.* 3.99745 × 3.99785 × 5.99502 × .78539816, which he makes 74.94823 cubic inches, inſtead of 75.247149, which is the real product. This naturally ſuggeſted the propriety of examining the whole, and the reſults of this examination are what I ſhall now lay before the public through the medium of the Philoſophical Journal. The power of correctly appreciating and comparing ſpace and quantity are ſo eſſential to

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* Copied in the Quarto Series of this Journal, III. 197.

[illegible]
.6016265
.7777906
[illegible]
[illegible] [illegible]

every practical application of the mathematics to the purpoſes of real utility, that I have no heſitation in undertaking the humble office of commentator on ſo valuable a text.

Real cubic dimenſions of Sir G. S. Evelyn's ſolids.

II. It is ſtated in § 13* that the product of the three mean meaſures of the ſides of the cube, *viz.* $4.98882 \times 4.98955 \times 4.98925$, is 124.18917: this however is erroneous, it being in fact 124.192246. The ſolid content of the cylinder was 75.247149, as already ſtated in the preceding paragraph, and that of the ſphere (§ 28) $= \overline{6.00745}^3 \times .52359878 = 113.519147$ cubic inches.

Correction for temperature of the braſs.

III. Suppoſing the braſs ſcale and theſe bodies to be at the ſame degree of heat, their apparent dimenſions as meaſured by it would be the ſame whatever was the temperature of both; the ſcale being of the ſame ſubſtance, and conſequently expanding and contracting with the bodies themſelves. Their real dimenſions however varying with the temperature, we muſt aſſume ſome certain degree of heat at which the graduations on the ſcale are to be conſidered as meaſuring correſpondent intervals of ſpace. We will ſuppoſe this to be 60° of Fahrenheit's thermometer. Now in order to aſcertain the real cubic meaſure by the ſcale when at 60°, of the quantities of diſtilled water diſplaced by thoſe bodies when they were weighed in it, we muſt add to or ſubtract from the nominal cubic dimenſions of each, 3 millionth parts (vide Tab. I. in the note on § 23), multiplied by the number of degrees above or below 60°, at which they and the fluid were at each of thoſe times.

At the time of their being reſpectively weighed in water, therefore, the ſolid content of the cube at 60.2° (§ 22) was $124.192246 \times 1.00000062 = 124.192323$ cubic inches of the ſcale when at 60°; that of the cylinder at 60.5° (ſecond experiment, § 23) $75.247149 \times 1.0000015 = 75.247161$; and that of the ſphere at 66° (§ 29) $113.519147 \times 1.000019 = 113.521304$.

Correction neceſſary for denſity and temperature of the air.

IV. In appreciating the apparent difference of weight in a body when weighed in air or in vacuo, it has been uſual to aſſume, that air at a given temperature, and under a given preſſure, is always of the ſame ſpecific gravity. This we know is not the caſe; conſiderable variations in this reſpect being occaſioned by its different ſtates of dryneſs, electricity, and perhaps other cauſes: as, however, experiments are wanting for the correct eſtimation of theſe differences, we will take the firſt ſuppoſition to be true, and apply the corrections accordingly from Sir G. S. Evelyn's tables for this purpoſe, in his note ſubjoined to § 23.

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* Theſe references with a § prefixed, are to the ſections ſo numbered in Sir G. S. Evelyn's Paper, which it is thought proper to mention here to ſave the neceſſity of repetition.

Air expands .0027 for 1° of Fahrenheit.

This gentleman tells us in the Philoſophical Tranſactions for 1777, that the ſpecific gravity of air at 51°, under a preſſure equal to 29.27 inches of mercury, is to that of water as 1 to 836, or = .001208; taking the latter as unity. Aſſuming therefore that the expanſion of air, for each degree of Fahrenheit's thermometer between 51° and 60°, is .0027 of the whole, we get the ſpecific gravity of air at 60 under the like preſſure = .001179, and 29.27 : .001179 : : 29.5 : .001188 = the ſpecific gravity of air at 60° when the barometer ſtands at 29½; and a cubic inch of ſuch air conſequently weighs $\frac{3}{10}$ of a grain very nearly.

Sp. gr. of air at 60°, bar. 29.5, = .001188 = .3 gr. for each cubic inch.

Denſities of the air in which the bodies were weighed.

Now the cube was weighed in air at 62° under a preſſure of 29 inches (§ 20); the cylinder in air at 62° under a preſſure of 29 inches (§ 21); and the ſphere in air at 67° under a preſſure of 29.74 inches (§ 29).

And the exceſſes of the weights of the reſpective quantities of diſtilled water diſplaced by theſe bodies, on their being afterwards weighed in this fluid, over thoſe of ſimilar quantities of air at the ſeveral temperatures and denſities above-mentioned, were as follow; *viz.*

The apparent weights of equal bulks of water.

That of the water diſplaced by the cube (§ 22), 32084.82 − 703.03 = 31381.79 grains;

That of the water diſplaced by the cylinder (ſecond experiment § 23), 21560.05 − 2553.22 = 19006.83 grains;

And that of the water diſplaced by the ſphere (§ 30), 28673.51 grains.

Correction for preſſure and temperature applied.

The air in which each of the bodies was weighed being, however, lighter than air at 60° when the barometer ſtands at 29½, we muſt correct theſe exceſſes of weight in the ſeveral quantities of water, ſo as to find how much the weight of each of them exceeds that of an equal quantity of air of ſuch ſtandard ſpecific gravity; or in other words, how much the weight of each of the bodies in water differs from that which it would have been found to poſſeſs in air in this latter ſtate. Theſe corrections for the denſity and temperature of the air we ſhall find, from the data before given in this article, to be as follows:

	Correction for	
	Preſſure.	Temperature.
	Gr.	Gr.
Of the weight of the water diſplaced by the cube - - -	– .642	– .201
Of the weight of the water diſplaced by the cylinder - - -	– .389	– .122
Of the weight of the water diſplaced by the ſphere - - -	+ .282	– .644

Correction of ſp. gr. of water when reduced to 60°.

The weights of theſe bulks of water, therefore, if weighed in air at 60° under a preſſure of 29½, would be found to be,

That diſplaced by the cube 31381.79 – .642 – .201 = 31380.95 grains;

That diſplaced by the cylinder 19006.83 – .389 – .122 = 19006.32 grains;

And that diſplaced by the ſphere 28673.51 + .282 – .644 = 28673.15 grains.

V. We have now, however, to correct theſe weights for the change which would take place in the ſpecific gravity of the water itſelf, by reducing it to 60° of temperature; that in which the cube was weighed having been at 60.2° (§ 22); that in which the cylinder was weighed at 60.5° (§ 23); and that in which the ſphere was weighed at 66° (§ 30).

Firſtly, by Sir G. S. Evelyn's table.

We will firſtly do this by the help of Sir G. S. Evelyn's Tab. I. in his note on § 23, according to which, a quantity of water of equal bulk with the cube would, at 60° of Fahrenheit's thermometer, weigh 31380.95 × 1.000033 = 31381.986 grains; one of equal bulk with the cylinder, 19006.32 × 1.000083 = 19007.897 grains; and one of equal bulk with the ſphere, 28673.15 × 1.00099 = 28701.536 grains; if weighed in air at the ſame temperature, when the barometer ſtands at 29 inches and a half.

The

Reſult.

	Grains by Sir G. S. Evelyn's Weights, or	Grains of the Parliamentary Standard Weights, which are thoſe uſed by him as 1.0006562 : 1. (§ 41).
The firſt gives the weight of a cubic inch of diſtilled water under theſe circumſtances $= \frac{31381.986}{124.192328} =$	252.689	252.523
The 2d $= \frac{19007.897}{75.247461} =$	252.606	252.440
And the 3d $= \frac{28701.516}{113.521304} =$	252.829	252.663

VI. It may perhaps however be proper to obſerve here, that the alteration in the ſpecific gravity of water by change of temperature, is eſtimated in this table of Sir G. S. Evelyn's (note on § 23), at nearly double that which is given by Mr. Gilpin in his tables, in the Philoſophical Tranſactions for 1794, p. 382; his appreciation of it from 60° to 66° (taking it to be equal to unity at 60°) being as follows; *viz.*

By Mr. Gilpin's tables:

61°	.99991	63°	.99971	65°	.99950
62	.99981	64	.99961	66	.99939

I am, from my own experiments on this ſubject, diſpoſed to prefer theſe tables; and taking them to be correct, we ſhall have a cubic inch by Sir G. S. Evelyn's ſcale when at the temperature of 60°, of diſtilled water alſo at 60°, weighed in air at 60°, under a preſſure of 29½ inches.

Ultimate reſult. A cubic inch of water at 60°, in air at 60°, bar. 29.5, = 252.506 grains;

By the cube = - -	252.519	Grains of the Parliamentary Standard according to Sir G. S. Evelyn's appreciation of it in § 41 of his Paper.
By the cylinder = - -	252.432	
By the ſphere = - -	252.568	
And by a mean of all three =	252.506	
And if weighed in vacuo inſtead of in air = - -	252.806	

or = 252.806 grs. in vacuo.

J. FLETCHER.

Cecil Street, Dec. 20, 1802.

VII. *On*

VII.

On the Quantities of Light afforded by Candles in Proportion to the Conſumption of Material and other Objects reſpecting the ſame. In a Letter from MR. EZ. WALKER.

To Mr. NICHOLSON.

SIR,

Probability that Candles which burn without producing ſmoke, will afford light in proportion to the matter conſumed.

WHEN a lighted candle is ſo placed as neither to require ſnuffing nor produce ſmoke, it is reaſonable to conclude that the whole of the combuſtible matter which is conſumed is converted to the purpoſe of generating light; and that the intenſities of light generated in a given time by candles of different dimenſions, are directly as the quantities of matter conſumed. That is to ſay; when candles are made of the ſame materials, if one candle produce twice as much light as another, the former will in the ſame time loſe twice as much weight as the latter.

Experiments in proof.

To prove the truth of this poſition, I made the experiments contained in the following

TABLE.

No. of the Experiment.	No. of the Candles.	Time of burning.	Weight of the Candles conſumed.	Strength of Light.	Diſtance of the Candles from the Wall.
		h. ′	oz. dr.		Feet.
1	1	3 0	0 15	1.	7
	3	3 0	1 1½	1. +	7
	Mould	3 0	0 15	1.	7
2	1	2 55	0 15	1	8
	3	2 55	1 0	1 +	8
	Mould	2 55	0 15	1	8
2	1	3 0	0 15¾	1	8
	3	3 0	1 2	1⅛	8¾
	Mould	3 0	1 0	1	8
4	5	3 0	1 5	1.18	8¾
	Mould	3 0	1 1⅛	1.	8

Theſe experiments were made in the following manner.

Method of experiment.

I took three candles, the dimenſions of which are given in the table, againſt 1, 3, and mould *. Theſe were firſt weighed, and then lighted at the ſame inſtant. At the end of the time inſerted in the third column of the above table, they were extinguiſhed and weighed again, and the loſs of weight of each candle is contained in the fourth column.

Obſervations on the reſults.

The three firſt experiments were made under ſuch favourable circumſtances, that I have little doubt of their reſults being more accurate than what practical utility requires, but the fourth experiment cannot be depended on ſo much, in conſequence of the variable light of No. 5. This candle was moved ſo often to keep the two ſhadows equal, that I was under the neceſſity of ſetting down its mean diſtance from the wall by eſtimation; but as this was done before the candles were weighed, my mind could not be under the influence of partiality for a ſyſtem.

Intenſity of light determine by the method of ſhadows.

The method which I uſed in comparing one light with another in each experiment, was nearly that which has been already deſcribed †. The only difference conſiſted in having the obſtacle which formed the ſhadows fixed, inſtead of being held in the hand. A two feet navigation ſcale, which was made faſt to the ſtand of a ſmall teleſcope in a perpendicular direction, and ſet upon a table near the wall on which the ſhadows were compared, reduced the labour of making the experiments very much, and gave me an opportunity of making a greater number of obſervations in the ſame time than I could have done with the ſcale in my hand.

REMARKS ON THE EXPERIMENTS.

The ſtandard compariſon of experiments at different times was a mould candle by the weight conſumed, of whic the lights were infered.

1. The experiments were made at different times, and the light of the mould candle was made the ſtandard, with which the lights of the others were compared; but it muſt not be underſtood, that this candle gave the ſame ſtrength of light in every experiment.

Other remarks.

2. The ſign + in the 5th column, ſignifies that the candle againſt which it is placed, gave a ſtronger light than the others

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* Philoſophical Journal, octavo, III. 273.

† Philoſophical Journal, quarto, I. 67. and Philoſ. Journal, octavo, III. 275.

in the ſame experiment, but the exact quantity being ſmall, I did not aſcertain by calculation.

3. The mould candle in the 4th experiment, loſt more of its weight in three hours, than in the preceding experiments. This was owing to the current of air in the room in which the experiments were made, being greater than uſual, occaſioned by a freſh gale of wind.

GENERAL LAWS.

General law as at firſt ſtated.

From the experiments contained in the table, it appears to be an eſtabliſhed law, where combuſtion is complete, that the quantities of light produced by tallow candles, are in the complicate ratio of their times of burning and weights of matter conſumed.

For if their quantities of matter be equal, and times of burning the ſame, they will give equal quantities of light, *by the experiments.*

And if the times of burning be equal, the quantities of light will be directly as their weights of matter expended, *by expements 3 & 4.*

Therefore the light is univerſally in the compound ratio of the time of burning and weight of matter conſumed.

STANDARD OF LIGHT.

More ample explanation reſpecting the ſtandard candle.

If the law which I have endeavoured to prove, both by reaſon and experiment, be admitted, we have a ſtandard by which we may compare the ſtrength of any other light.

Let a ſmall mould candle, when lighted, be ſo placed as neither to produce ſmoke nor require ſnuffing, and it will loſe an ounce of its weight in three hours. Let this quantity of light produced under theſe circumſtances, be repreſented by 1.00.

Then ſhould this candle at any other time, loſe more or leſs of its weight in three hours than an ounce, the quantity of light will be ſtill known, becauſe the quantity of light in a given time is directly as the weight of the candle conſumed, *by the general law.*

CONCLUSION.

Advantages of the inclined candle;

A candle which is uſed in the manner that I have pointed out, gives *more light* than a candle of the ſame dimenſions ſet perpendicularly and ſnuffed, becauſe one part of a candle that is ſnuffed is thrown away, and another part flies off in the form of ſmoke. But this is not the only inconvenience that attends the uſing of a candle in this manner, and which the other method is free from, for the light which it gives is of a bad quality, on account of its being variable and undulating.

From the time that a candle is ſnuffed till it wants ſnuffing again, its ſtrength of light ſcarcely continues the ſame for a ſingle minute. But that variation which frequently takes place in the height of the flame, is a matter of ſtill more ſerious conſequence.

contraſted with the upright candle, which requires ſnuffing.

The flame of a long candle, when it burns ſteadily, is about two inches high, but it very frequently riſes to the height of four inches or upwards; drops down again in a moment, till it is leſs than three inches, and then riſes again. In this manner the flame continues in motion for ſome time before it returns to its original dimenſions. But it does not continue long in a quieſcent ſtate before it begins a new ſeries of undulations. In this manner the candle burns till the top of the wick is ſeen near the apex of the flame, carrying off clouds of ſmoke. In this ſtate of things the eye becomes uneaſy for want of light, and the ſnuffers are applied to remove the inconvenience.

Extreme undulations of large candles placed vertically;

It is theſe ſudden changes, and not the nature of candle light itſelf, that do ſo much injury to the eye of the ſtudent and artiſt; but that injury may be eaſily prevented, by laying aſide the ſnuffers, and in the place of one large candle, let two ſmall ones be uſed in the manner which I have before taken the liberty to recommend.

and the miſchievous conſequences to the eyes.

I remain,

SIR,

Your's reſpectfully,

EZEKIEL WALKER.

Lynn Regis, Dec. 20, 1802.

Deſcription

VIII.

Deſcription of an Engine for raiſing and lowering Weights by the Action of a Column of Water and for other purpoſes. By JOHN HARRIOTT, *Eſq.*

To Mr. NICHOLSON,

Dear SIR,

I SEND the drawings and deſcriptions of my ſyphon engine, which from its power and great convenience in actual practice, will I preſume be thought a fit ſubject for your valuable publication.

Engine acting by a column of water acting alternately on the upper and under ſurface of a piſton, as directed by turning a cock.

A A. In Fig. 1 and 2, is a cylinder with a moving piſton therein, of which D is the piſton rod.

B and C. Are water ways through which the water is admitted to communicate with both ſides of the piſton.

E F. A pipe in Fig. 1, through which water deſcends from a reſervoir above, into a three way cock M. and in Fig. 2, is a pipe through which any ſtream or head of water runs to the three way valve in the ciſtern M.

C H. Is a pipe in both, communicating from the three way cock, or valve, to the upper part of the cylinder.

K B. Is a pipe communicating from the ſame cock or valve, to the lower part of the cylinder.

I I. Is a pipe communicating between the two laſt mentioned pipes, conſequently between the upper and lower ſpaces of the cylinder, which communication can be either cut off or opened to any requiſite degree by the cock L.

N. Is a pipe in which a lower column of water is ſuſpended by the reaction of the atmoſphere, and conſequently a power to the upper column, or fall in proportion to its length or depth, not exceeding the weight of the atmoſphere.

The column conſiſts partly of an actually preſſing maſs and partly of a ſuſpended portion, which renders the atmoſphere active on the machinery.

The nature and principle of the ſyphon engine conſiſts in combining the power of the ſyphon with the direct preſſure of a column or ſtream of water, ſo that they may act together. It works by means of the ſyphon conſtantly acting in concert with the feeding ſtream of water, ſo that each alternately act on the upper and lower part of a piſton, within a cylinder as it were, reverſing the ſyphon at each change; and the power is equal to a column of water of the ſame diameter as that of the cylinder, and equal in length to the height of the head, above the tail water. For inſtance, if a column of water of any given diameter has a fall of 20 feet until it reaches an engine, its power is clearly aſcertained. Now whatever that power is, if a ſyphon pipe be added to this engine, ſo as to connect with the column, and the ſyphon pipe has alſo a fall of an equal length; namely, 20 feet to the lower end, which is immerſed in water, the engine although placed in the midway, will then have a power equal to that of a deſcending column of 40 feet, and ſhould the column or fall to the engine be but two feet, and the lower ſyphon pipe 24 feet, the power would be equal to a fall of 26 feet; and in this manner in every various diverſity between the falling column and the ſyphon pipe beneath, the latter will produce an equal power according to its proportionate length, or depth to the ſurface of the tail water, provided it does not exceed above 30 feet, or the weight of the atmoſphere; and where a ſtream of water is either level with, or even below, the place at which it is deſirable to fix the engine, there will be no difficulty in placing it either below, or on the level, or above the ſtream itſelf, provided the height where it is fixed above, does not exceed 28 or 30 feet, and the place where the water flows off be ſtill lower. The conſtruction may evidently be varied according to the local ſituation and circumſtances of applying it, and the uſe to which it may be adapted, in giving activity to different kinds of machinery.

Engine to be uſed inſtead of a crane. Its advantages ſtated.

The drawing, Fig. 1, exhibits the apparatus for raiſing or lowering weights of any kind, on wharfs or in warehouſes. A man or boy can raiſe or lower goods of any weight, without other exertion than that of merely turning the three way cock M. to an index; in either raiſing or lowering, the *ſtop* is inſtantaneous, by a ſmall motion or turning the cock to the ſtop mark in the index: this moſt effectual of ſtops, or gripe, operates ſo quietly and eaſy without any jirk, or jarring, that it removes the uſual riſk attending common cranes or machinery in which men are ſometimes overpowered. It raiſes and lowers goods with thrice the velocity uſually produced by manual labour, yet an engine of dimenſions ſufficient to raiſe ſeveral tons, may be ſo graduated by the perſon at the cock, as to bring it to the ſmootheſt ſloweſt motion poſſible. The ſaving of labour and time muſt therefore be conſiderable, the riſk of plunder diminiſhed, and delays in ſetting to work for want of help removed.

Though this engine requires a reſervoir of water; yet this may eaſily be had and will be highly beneficial in caſes of fire.

The great and only obſtacle to its general application in this way, is the want of a natural head of water in moſt ſituations of warehouſes; and the queſtion in theſe caſes would be, whether it would be worth while to raiſe a head or reſervoir of water for ſuch purpoſes. If this be done, *one* reſervoir would work any number of engines at any diſtance by one main pipe, and as many branch pipes as there were engines. The expence of a ſmall ſteam engine to raiſe the ſame water (or any other) to the reſervoir, would be more than repaid by the ſecurity it would give againſt fire. A gallon of water at the firſt alarm of fire, has more effect in extinguiſhing it, than a hogſhead after it has got a-head. To produce this effect, and apply it in the ſpeedieſt manner wherever ſuch reſervoir is, would only require ſmall pipes to be led from the main pipe that ſupplies the engines, to the warehouſes, or where it may otherwiſe be wanted, and at the end of ſuch pipe, a ſcrew nozzle and a cock to turn on or off, a leather hoſe and branch pipe, ſuch as all fire engines have, being provided and hanging near. One man only at the firſt alarm of fire, would in a few minutes ſcrew the hoſe upon the ſcrew nozzle, and then turning the cock, he would of himſelf be able to play upon the fire with all the force of a ſtrong fire engine.

Propoſal for public reſervoirs of water in every pariſh.

Perhaps it is not too much to ſay, that this ready preventative in caſe of fire, would be equal in effect to an inſurance. Independent therefore of its application to the ſyphon engine for raiſing and lowering weights, I have conceived, that if ſuch reſervoirs of water were provided in all cities and towns, well elevated at ſuitable diſtances (one or more in every pariſh) there would not be near the danger and calamity from fire that now exiſts. The diſtance of the reſervoir from the fire is of no conſequence, provided the pipes were laid with ſufficient water way; and it would ſeldom happen that more than one fire at a time would break out in the ſame pariſh, or if it did, the ſame ſpeedy and effectual aſſiſtance could be given by any number of ſuch powerful and ſelf-working engines, as long as there was water in the reſervoir. And even ſuppoſing the ſupply from the reſervoir to be limited to any given time of expenditure, common prudence would lead to its being repleniſhed by the uſual mode, before it could be entirely exhauſted.

The

Other uſes and applications of the engine.

The drawing Fig. 2, ſhows how the ſyphon engine is to be applied to ſtreams of water, the advantages of which are, that the engine as well as the mill work, or manufacturing machinery it may drive, may be placed where moſt convenient, above or below the head or ſtream, to be worked by a fall of water from the leaſt to the greateſt height, or by any ſtream or river, the tail water below acting and having as much power as the head, anſwering to the height of either. Nor can a drop of water eſcape without performing its full duty. The power is therefore greater, and not liable to the diſadvantages attending a water wheel. In tide waters it would work ebb and flood ſo long as there was a difference of two feet or leſs in the height, regulating itſelf, ſo that the power may be equal, let the head and tail water riſe or fall, by which a ſmooth uniform motion is maintained and adapted to the ſmalleſt as well as the ſtrongeſt power wanted. If the tail water re-acts upon a water wheel, it muſt loſe ſo much of the power of the fall, or in other words, whenever the tail water riſes above the loweſt waſh boards of the wheel, a counter action will be exerted as is well known againſt the power of the wheel. A conſiderable quantity of the ſtream muſt likewiſe eſcape without any good effect. Theſe diſadvantages of the water wheel, are removed by the ſyphon engine; and in froſty weather being fed by a pipe from below the ice, it will not be impeded, becauſe its velocity in paſſing through the engine, will prevent the water being expoſed a ſufficient time to the cold atmoſphere to congeal it; and when at reſt the engine may be left empty. It is ſcarce neceſſary to obſerve, that when the engine is fixed below the running head of the ſtream, it will fill as ſoon as the ſluice is opened, and ſet itſelf to work; but when placed above the head, it will require ſufficient water to fill the whole interior ſpace, which being thus charged and converted into a ſyphon, will then work as well and with as much power as if the engine were placed below the head of the ſtream.

Froſty weather affects it leſs than mills and other hydraulic machines.

I am,
Dear SIR,
your moſt obedient and
very humble ſervant,
JOHN HARRIOTT.

Thames Police, Dec. 20, 1802.

REMARKS.

REMARKS.—W. N.

Conſideration of the two principal objections to a water engine inſtead of a crane.

Labour of raiſing the Water,

compared with that now employed to hoiſt and lower goods.

Men.

Horſes.

Steam.

The labour ſaved by Mr. Harriott's engine (ſupplied by ſteam) would exceed ſix ſevenths of that now expended.

The convenience and facilities afforded by Mr. Harriott's Engine, will be obvious to every one; and the only circumſtances which can be urged in the way of objection againſt it, are thoſe which he has himſelf noticed; namely, that it in general requires the water to be raiſed, which is to work it as a crane, and that this water is ſubject to freeze. To meet theſe difficulties fairly, we muſt admit, that if a ton of water were raiſed to the top of a warehouſe, in order by its fall to raiſe a ton of goods (even if it could do that) it would be ſimpler for the warehouſeman to raiſe his goods without the intervention of the fluid. But while we admit this general truth, it muſt alſo be obſerved, that the poſitions do not include the whole of the actual caſe. The raiſing and lowering of goods being a proceſs of conſiderable ſkill and intelligence, and being neceſſarily carried on with many ſtops, interruptions, and variations of force, is on theſe accounts performed by the moſt expenſive of all firſt movers; namely, the ſtrength of men, and even this power is for a large part of the time inactive; namely, during all the intervals of operation. Here then is a wide field for the ſaving of force, if it could be ſtored up and uſed when wanted. Suppoſe for example, that two men were employed in a crane, and that the pauſes of inactivity amounted to one third of their whole time, theſe men would be more beneficially employed in raiſing water, to be afterwards directed by a boy, or by the foot of a clerk who ſtood by, to keep account of the delivery of packages. But as horſe work is reckoned at leaſt five times as cheap as human labour, the ſaving of labour by employing that animal, would be about ſix ſevenths of the whole; and ſteam engine work upon a very ſmall ſcale indeed, would be as cheap again as horſe work. Hence it appears, that after every allowance for the greater quantity of water required to produce velocity by its fall, and for other circumſtances the ſaving of labour muſt be very great, by thus œconomizing and ſtoring up the force, excluſive of the conveniences detailed by Mr. Harriott in his letter.

Froſt would moſt probably affect it in mill work as it does other engines;

The freezing of the water is an impediment of ſuch a nature, that it will perhaps be found that the operations of natural ſtreams or falls of water would be ſubject to interruptions from this cauſe, with his engine about as ſoon as with others that are more expoſed, ſo that its advantages would conſiſt of the other particulars which he has detailed. But with the engine applied for raiſing goods, it is to be apprehended that the water in the elevated reſervoir would be bound up by freezing, and alſo in ſevere weather checked if not confined by the ſame cauſe in the engine itſelf. The remedy for this appears to be to work the engine, by raiſing the ſame maſs of fluid repeatedly, and diſſolving ſome cheap material in it which ſhould render it leſs diſpoſed to freeze. Moſt ſaline bodies would have this effect, and the quantity once added, would remain long without waſte or loſs. Experiment would ſhew what ſalt, and whether earthy, neutral, metallic, alkaline, or acid, might be the cheapeſt, moſt effectual, and leaſt diſpoſed to act upon the engine *.

but in crane-work the water may be rendered incapable of freezing in our climate;

by a moderate addition of ſome cheap ſaline ſubſtance.

IX.

On the Electricity of the Shavings of Wood.

By Mr. W. WILSON.

SIR, *London, Dec.* 13, 1802.

YOUR readineſs to inſert my letter of the 11th of October, encourages me to requeſt the inſertion of the following account of experiments on the electricity obtained by cutting of wood, &c. In doing which you will very much oblige

your obedient humble ſervant,

WM. WILSON.

HAVING frequent occaſions to work very dry wood that has lain over a large fire for ſeveral hours, I have often obſerved the ſhavings, &c. to adhere to the tools, and whatever they touched. About two years ago I began to take particular notice of, and endeavour to find the cauſe of this phe-

Wood ſhavings adhere to the tools.

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* In the conſideration of this engine, the attention of the reader will be directed to the preſſure engine of Mr. Trevithick, in our firſt volume.

nomenon, and after ſatisfying myſelf that it was not occaſioned by any moiſture or roughneſs on the ſubſtance; I ſuſpected that electricity was the cauſe, and I accordingly ſet about the experiments which are the ſubject of this communication.

Dry warm beach ſhaved with glaſs is poſitive,

I laid a circular tin plate, 6 inches in diameter, on the cap of Bennet's electrometer, and with a piece of dry and warm window glaſs, ſcraped a piece of dry and warm beach, a few of the ſhavings being received on the tin plate, made the gold leaf ſtrike the ſides of the bottle with poſitive electricity. This was always the caſe whether the wood was hot or cold, but not always equally ſtrong.

with ſteel negative.

I thought a knife would be more convenient than glaſs to ſcrape with, and when I tried it, I found the ſhavings were negatively electrified, although they were taken from the ſame piece of wood which before gave poſitive.

Other woods.

This change in the reſult induced me to try different woods in different ways (ſometimes ſcraping and ſometimes cutting ſmall chips) but obtained very uncertain reſults, for ſometimes I obtained poſitive and ſometimes negative electricity, even when I cut the ſame piece of wood with the ſame knife.

An inſulated knife acquires the contrary electricity.

I next fixed the blade of a penknife into a glaſs tube, covered with ſealing wax, and ſet to work with this inſulated knife, and found that it was always electrified with the contrary electricity to that of the chips which were moſt frequently poſitive. But as they were ſometimes negative, I repeated the experiments very often to diſcover the cauſe, but with very little ſatisfaction.

A ſharp inſulated knife gave negative chips; a blunter poſitive.

However, after making ſeveral hundred trials, I found the keenneſs of the edge of the knife had ſome influence, for one day after chipping with the inſulated knife, and getting poſitive chips, I ſet the knife on a hone to make it cut better (which I had frequently done before) and when I began to chip the ſame piece of wood which but juſt before gave poſitive chips, I found the chips were negatively electrified ſeveral times, I then chipped the ſame piece of wood with a knife that had been very much uſed, without ſharping, and this gave poſitive chips as the other had done before it was ſharped; the knife that had been ſharped was tried again, but the chips were poſitive now, however when it was ſharped again it gave negative chips.

I now

Other contradictory experiments.

I now thought I had found the cauſe of the contradictory reſults, but to be more certain about it, I began the following ſet of experiments. I ſharpened a penknife to a very nice edge, and uſed the ſame pieces of wood that had been uſed in the former trials. In 24 trials with cherry-tree, the chips were always negative, and in four trials with elm, and in ſix with yew, the chips were always negative. I now drew the edge of the knife lightly over a piece of iron to dull it, expecting to get poſitive chips, but on trial the chips were ſtill negative. Suppoſing the knife was not dull enough, I drew the edge over the iron again, and made the edge very bad, but the chips were ſtill negative. The edge of the knife was next made rough by rubbing it on a grindſtone, and this rough edge gave negative chips. The knife was next ground and ſet on the hone very carefully, and this ſharp edge gave negative chips.

Probability that the heat of the wood might influence the reſults.

As nothing ſatisfactory was obtained from this ſet of experiments. I began to ſuſpect that the degree of heat of the wood was to be conſidered (for the wood was cold in all the laſt experiments, and ſometimes hot, and ſometimes cold, but moſt frequently hot in the firſt) or that perhaps the heat of the wood, and the ſharpneſs of the knife, were both to be taken into the conſideration. I therefore ſet about the following ſet of experiments.

Detail of experiments with hot and cold wood; and different ſharpneſs of the knife.

I ſplit the piece of cherry tree (that was uſed before) into two pieces, one of which was made thoroughly hot at the fire. This when chipped with the ſame knife that was uſed in the laſt experiments, without being ſharpened, gave poſitive chips every time in ſix trials, and after this piece of wood had cooled till it was ſcarcely warm, gave poſitive chips every time in four trials. I then took the other piece which had not been near the fire for five or ſix hours, this gave negative chips every time in four trials. I now made this piece of wood quite hot, thoroughly, and chipped it again with the ſame knife, and in ſeven trials the chips were poſitive every time. Theſe two pieces of wood was now laid by for three or four hours to get quite cool. In this ſtate they gave negative chips every time in twelve trials. One of them when made thoroughly hot again, gave poſitive chips every time in ſix trials. The other piece was now made warm (but only externally ſo) in eight trials it gave poſitive chips four times, and negative chips four times; but after laying three or four hours to cool it gave negative chips every time in eight trials. A third piece of cherry-tree that had not been near a fire for four or five days gave negative chips. I repeated theſe experiments with different knives that had not very ſharp edges, and with beach as well as cherry-tree. And whenever the wood was made thoroughly hot at the fire, it always gave poſitive chips, not only when hot, but when it was ſo cooled as not to be ſenſibly warm; but when it had laid away from the fire three or four hours, the chips were always negative. Sometimes when the wood was but ſlightly warmed, it would be very difficult to get any ſigns of electricity, and at other times when the wood was made hot only externally (by putting it very near the fire for a ſhort time) the firſt few chips would be poſitive, and the ſucceeding ones negative. I had one inſtance where with the firſt chip the electrometer diverged near an inch, and with the ſecond it completely cloſed again. Having ſucceeded thus far, I thought I would try whether the reſults would be the ſame if I uſed a very ſharp knife, and accordingly ſharped two knives on a hone very carefully. And I uſed the ſame pieces of cherry-tree made thoroughly hot; in nine trials with one of the knives, the chips were negative every time and in five trials with the other knife, the chips were negative every time. I made a number of ſimilar trials with a piece of beach with always the ſame reſults, but the beach ſeemed to be too hard for that keenneſs which is neceſſary to produce negative chips, for after cutting one or two chips, the edge would be ſpoiled, and produce poſitive chips; but always when the knife was ſharped, the firſt one or two chips would be negative. Similar to this I found by ſubſequent trials to be the caſe with the pieces of cherry-tree, ten or twelve chips of this (which was very ſtraight and open grained) would ſpoil the edge.

General reſults. Shavings of dry wood by glaſs, are negative—hot wood by a moderate ſteel edge is poſitive, but negative if cold:—and a very ſharp edge gives negative whether hot or cold.

From theſe experiments it appears, that when very dry wood is ſcraped with a piece of window glaſs, the ſhavings are always poſitively electrified. And if chipped with a knife, the chips are poſitively electrified if the wood is hot, the edge of the knife not very ſharp, and negatively electrified if the wood is quite cold. But if the edge of the knife is very keen, the chips will be negatively electrified whether the wood is hot or cold.

The greateſt number of trials was made with the inſulated knife, which was always electrified contrarily to the chips; but the ſurface of the wood where the chips were cut from was very ſeldom electrified, and when it was it was always but weakly ſo, and of the ſame denomination as that of the weakeſt of the other two. I have repeatedly found that if a piece of dry and warm wood is ſuddenly ſplit aſunder, the two ſurfaces which were contiguous are electrified, one ſide poſitive and the other negative.

The knife always contrary.

Split wood has the two ſtates.

X.

On the Compoſition of Emery. *By* Smithson Tennant, *Eſq. F. R. S. (Ph. Tranſ.* 1802.)

THE ſubſtance called emery, which, from its great hardneſs, has been long uſed in various manufactures, for grinding and poliſhing other bodies, has not, it appears, been hitherto correctly analyzed. In books of mineralogy, it is conſidered as an ore of iron; an opinion probably derived from its great ſpecific gravity, as well as from the iron which it frequently contains. But, where this metal is moſt abundant, it could not be extracted from it with advantage, and ought rather to be regarded as an impurity, as it does not contribute to produce the peculiar hardneſs for which this ſubſtance is diſtinguiſhed. In Mr. Kirwan's mineralogy, he mentions an examination of emery made by Mr. Wiegleb, from which he inferred that 100 parts conſiſted of 95,6 of ſilex, and 4,4 of iron. Mr. Kirwan, however, juſtly ſuſpects the correctneſs of this account, and obſerves that he had no doubt but ſome other ſtone was impoſed on Mr. Wiegleb for emery.

Emery has not yet been correctly analyzed.

When powder of emery is boiled in acids, it becomes of a lighter colour, from the loſs of part of the iron; after which, it does not ſeem to undergo any further alteration. As acids produce ſo little effect on it, I expoſed it to a pretty ſtrong red heat, with carbonate of ſoda, in a crucible of platina. On adding water to the maſs contained in the crucible, the greater part of the emery was found in powder; having only become of a light colour, from the extraction of part of the iron.

Emery powder gives part of its iron to acids.

Soda by fuſion diſſolved ſome argil and leaves the emery.

iron. Though this proceſs was twice repeated with the remaining powder, and in a ſtronger heat, a great porportion of it remained undiſſolved.

The alkaline ſolution, after a red calx of iron had ſubſided from it, was ſaturated with acid; and gave a precipitate of a white earth, which I found to be almoſt purely argillaceous.

This reſult is ſimilar to Klaproth's with diamond ſpar.

The reſult of theſe experiments, was ſo ſimilar to thoſe of Mr. Klaproth on diamond ſpar, as to render it very probable that emery was in reality the ſame ſubſtance, though uſually mixed with a larger proportion of iron; and the ſubſequent experiments appear to confirm this opinion.

Emery pulveriſed and cleared of magnetic particles.

In order to obtain a quantity of emery as free from iron as I could, I reduced to a coarſe powder, a piece which conſiſted of different ſtrata, ſome of which were of much lighter colour than others; and afterwards ſeparated, by a magnet, the particles which were attracted by it. The part which was not attracted by the magnet, I obſerved to have the uſual degree of hardneſs (by the ſcratches which might be made with it on flint.) I then reduced it to a finer powder, in an agate mortar; and, as this was principally done by preſſure, and not by grinding, hardly any ſenſible addition was made to its weight. In the ſame manner, I found that diamond ſpar might be powdered to the ſame degree of fineneſs, without any material increaſe of weight from the mortar.

20 of the clear powder fuſed with 120 ſoda,

Of the emery powder thus prepared, 20 grains were taken, and heated in the manner before deſcribed, with 120 grains of ſoda, which had been previouſly deprived of carbonic acid, and boiled to dryneſs in a ſilver pan. By nearly the ſame proceſs as that uſed by Mr. Klaproth, I obtained about 16,0 grains of argillaceous earth, ,6 of ſiliceous earth, ,8 or ,9 of iron, and ,6 of a grain remained undiſſolved. Theſe numbers, reduced to parts of a hundred, are therefore,

and by the ſame proceſs as Klaproth's the component parts of emery

Argillaceous earth - - - - -	80
Silex - - - - - -	3
Iron - - - - - -	4
Undiſſolved - - - - - -	3
	90.

Mr.

proved nearly the ſame as thoſe of Chineſe corundum.

Mr. Klaprorh obtained from the Chineſe corundum, after ſeparating from it the particles which were attracted by the magnet,

Argillaceous earth - - -	84
Silex - - - -	6,5
Iron - - - - -	7,5
	98.

As this analyſis was no doubt conducted with greater care than mine, the loſs of weight was leſs; but the proportion of the ingredients is ſufficiently near to ſhow that the ſubſtances are eſſentially the ſame.

Other emery containing one third part iron.

From 25 grains of emery which appeared the moſt impregnated with iron, and yet retained its uſual hardneſs, I obtained, argillaceous earth 12,5, ſilex 2, iron 8, and one grain was not diſſolved; or, per cent.

Argillaceous earth - - - -	50
Silex - - - - -	8
Iron - - - -	32
Undiſſolved - - - -	4
	94.

As ſuch emery can eaſily be had of uniform quality in large pieces, I procured the powder employed in this experiment, by rubbing two pieces againſt each other.

Another analyſis.

From 25 grains of emery, ſimilar in appearance to the preceding, but which had been digeſted with marine acid previous to the action of the alkali, I had,

		per cent.
Of argillaceous earth - -	16,4	65,6
Siliceous earth - - -	,8	3,2
Iron - - - - - -	2,	8,
Not diſſolved - - - -	4,5	18,0
	23,7	94,8.

Emery appears as hard as diamond ſpar.

The hardneſs of emery, as far as I could judge by its cutting rock cryſtal and flint, appeared to be equal to that of diamond ſpar. The latter could not be ſcratched by the former; but, as emery has not a ſurface ſufficiently poliſhed to render a mark viſible, the converſe of this could not be tried.

All

Emery comes chiefly from Naxos.

All the emery which is uſed in England, is ſaid to be brought from the Iſlands of the Archipelago, and principally from Naxos. In thoſe places, it is probably very abundant; as the price of it in London, which I was told was 8 or 10 ſhillings the hundred weight, appears little more than ſufficient for the charges of carriage. Though I ſaw a very large quantity in one place, (more than a thouſand hundred weight,) I could not find any pieces of a cryſtallized form; poſſibly the great proportion of iron uſually mixed with it, may prevent its cryſtallization. The whole conſiſted of angular blocks incruſted with iron ore, ſometimes of an octaedral form, with pyrites, and very often with mica. The latter frequently penetrates the whole ſubſtance of the maſs, giving it, when broken, a ſilvery appearance, if ſeen in the direction in which the flat ſurfaces preſent themſelves to the eye. As theſe ſubſtances have no chemical relation to the emery itſelf, it is remarkable that they ſhould alſo accompany the diamond ſpar from China; for Mr. Klaproth obſerves, "that its lateral "facets are moſtly coated with a firmly-adhering cruſt of "micaceous ſcales, of a ſilvery luſtre:" he alſo mentions, beſides felſpar, pyrites, and grains of magnetic iron ore.

It is cheap; not cryſtallized; is accompanied by the ſame ſubſtances as Chineſe diamond ſpar.

XI.

Experiments and Obſervations on the Power of Fluids to conduct Heat; with Reference to Count Rumford's Seventh Eſſay on the ſame Subject. By JOHN DALTON *.

The properties of heat are continually under our notice.

THE nature and properties of fire or heat are ſubjects which preſent themſelves to our conſideration in almoſt every department of phyſics: it is no wonder therefore that new experiments, which point out and define the modes of operation of fire, before unobſerved, or at leaſt too much overlooked, ſhould attract the attention of philoſophers.—Theſe obſervations were ſuggeſted upon reading Count Rumford's very ingenious experiments, in his eſſay above-mentioned, which exhibit a fact in a more ſtriking point of view than it has appeared

Count Rumford's experiments on the tranſmiſſion of heat by circulation through fluids.

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* Mancheſter Memoirs, vol. v. 373.

peared before—namely, *that the quickneſs of the circulation and diffuſion of heat in fluids, is occaſioned principally by the internal motion ariſing from a change of ſpecific gravity affected by the heat.*—But the concluſion he has drawn from them—*that fluids are perfect non-conductors of heat*, in the way in which ſolids conduct it, appears to me totally unwarranted from the experiments, and erroneous in itſelf. And as it may be an error of practical conſequence, if adopted, the expoſition of it ſeemed deſirable—which is the object of the following remarks and experiments.

His inference, that fluids are non-conductors erroneous.

Experiments.

My firſt attempt was to aſcertain the preciſe degree of cold at which water ceaſes to be further condenſed—and likewiſe how much it expands in cooling below that degree to the temperature of freezing, or 32°. For this purpoſe I took a thermometer tube, ſuch as would have given a ſcale of 10 inches with mercury from 32° to 212°, and filled it with pure water. I then graduated it by an accurate mercurial thermometer, putting them together in a baſon filled with water of various degrees of heat, and ſtirring it occaſionally: as it is well known, that water does not expand in proportion to its heat, it does not therefore afford a thermometric ſcale of equal parts, like quickſilver.

A thermometer made with water.

Greateſt contraction at 42°½.

Law of expanſion.

It is the ſame on each ſide above and below 42°½.

From repeated trials agreeing in the reſult, I find, that the water thermometer is at the loweſt point of the ſcale it is capable of, that is, water is of the greateſt denſity at 42½° of the mercurial thermometer. From 41° to 44° incluſively the variation is ſo ſmall as to be juſt perceptible on the ſcale; but above or below thoſe degrees, the expanſion has an increaſing ratio, and at 32° it amounts to $\frac{3}{8}$th of an inch, or about $\frac{1}{180}$th part of the whole expanſion from 42°½ to 212° or boiling heat.—During the inveſtigation of this ſubject, my attention was arreſted by the circumſtance, that the expanſion of water was the ſame for any number of degrees from the point of greateſt condenſation, no matter whether above or below it: thus I found that 32°, which are 10°½ below the point of greateſt denſity, agreed exactly with 53°, which are 10°½ above the ſaid point; and ſo did all the intermediate degrees on both ſides. Conſequently when the water thermometer ſtood at 53°, it was impoſſible to ſay, without a knowledge of other circumſtances, whether its temperature was really 53°, or 32°. Recollecting ſome experiments of and the water may be cooled down as low as 5° or 6° without freezing, &c. &c.

Dr. Blagden in the Philoſophical Tranſactions, from which it appears that water was cooled down to 21° or 22° without freezing, I was curious to ſee how far this law of expanſion would continue below the freezing point, previouſly to the congelation of the water, and therefore ventured to put the water thermometer into a mixture of ſnow and ſalt, about 25° below the freezing point, expecting the bulb to be burſt when the ſudden congelation took place. After taking it out of a mixture of ſnow and water, where it ſtood at 32° (that is 53° per ſcale) I immerſed it into the cold mixture, when it roſe, at firſt ſlowly, but increaſing in velocity, it paſſed 60°, 70°, and was going up towards 80°, when I took it out to ſee if there was any ice in the bulb, but it remained perfectly tranſparent: I immerſed it again, and raiſed it 75° per ſcale, when in an inſtant it darted up to 128°, and that moment taking it out, the bulb appeared white and opake, the water within being frozen: fortunately it was not burſt; and the liquid which was raiſed thus to the top of the ſcale was not thrown out, though the tube was unſealed. Upon applying the hand, the ice was melted, and the liquid reſumed its ſtation. This experiment was repeated and varied, at the expence of ſeveral thermometer bulbs, and it appeared that water may be cooled down in ſuch circumſtances, not only to 21°, but 5° or 6°, without freezing, and that the law of expanſion abovementioned obtains in every part of the ſcale from 42°½ to 10° or below; ſo that the denſity of water at 10° is equal to the denſity at 75°. But as the diſcovery of this curious, and I believe hitherto unnoticed property, has little to do with the object before us, I ſhall ſay no more of it at preſent.

(To be continued in our next.)

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Lectureſhip on Subjects of Natural and Experimental Philoſophy, at Newcaſtle upon Tyne.

Eſtabliſhment of a philoſophic lecture at Newcaſtle upon Tyne.

AMONG our provincial ſocieties for promoting the cultivation of literature and philoſophical purſuits, the ſociety at Newcaſtle upon Tyne, has for a number of years poſſeſſed an eminent ſtation for the ability of its members, and the unremitting aſſiduity and intelligence which they have exerted to diſcover and carry into effect thoſe objects which bodies of men are beſt capable of promoting. We all know that it is eaſy for a body of men to aſſemble together; to dignify themſelves by a name; to hold forth ſtriking pretenſions to the uninſtructed multitude; and to make a ſort of commercial traffic of that ſcientific celebrity which the voices of men can confer upon each other, for a time at leaſt, while the great public may aſk in vain what they have done to deſerve it. But when a body of true friends to their country and to mankind meet together, to conſider in what way they can beſt employ their talents for the good of ſociety and the promotion of the arts of civilized life, within that diſtrict which more immediately falls under the limit of their notice, and the extent of their power and influence;—when their well-earned celebrity is a ſecondary object, in compariſon with the internal ſatisfaction afforded by a proper employ of talents and virtue—ſuch an aſſociation becomes a public bleſſing, and its good conſequences extend to the remoteſt periods of after times. The Literary and Philoſophical Society of Newcaſtle, has already made inquiries into the ſtate of the arts, of agriculture, and of the mineral products of their vicinity. It has aſſiſted the diffuſion of knowledge by the eſtabliſhent of a well regulated and eaſily acceſſible library; and it has lately proceeded to conſtitute a lectureſhip on the ſubjects of natural and experimental philoſophy, the requiſite ſteps for which are now in progreſs with great ſpirit and activity.

Active exertions of the ſociety at Newcaſtle.

Hiſtory of the propoſed lecture.

Mr. Bigge's propoſal

On the fourth of May laſt, a paper by Mr. Thomas Bigge, on the expediency of eſtabliſhing a lectureſhip at Newcaſtle upon Tyne, upon the ſubjects of natural and experimental philoſophy, was read: In this paper, we find a clear, elegant adopted, and an addreſs to the public circulated.

and animated ſtatement of its objects; conſidered as well with regard to their great influence on ſociety, as to thoſe local circumſtances towards which the intention of the author was more particularly directed. In this paper, no leſs ſtriking for the value of its contents, than for the eſtimable motives of its author, we find facts which ſtrongly ſupport what has been ſaid at the commencement of this article concerning the general proceedings of this reſpectable ſociety. In conſequence of this ſuggeſtion, the ſubject was again reſumed, and an addreſs to the public was circulated in the month of June, requeſting their co-operation and aſſiſtance, exhibiting ſome part of the outline, and announcing that the appointment of Lecturer has been made to the Rev. Wm. Turner, a gentleman long and well known to the ſociety for the abilities with which he had exerciſed the office of ſenior ſecretary. Since that time, conſiderable progreſs has been made in the ſubſcriptions; and the lecturer delivered a general introductory diſcourſe on Tueſday, November the 16th laſt, upon the objects, the advantages, and the intended plan of the lectures. I am ſorry that the limits of a ſhort ntoice forbid the attempt to give any analyſis of this excellent diſcourſe, which affords ample evidence of the comprehenſive views of the author, with regard to the organization and duties of ſociety in general, as well as the ſciences he has undertaken to teach. I have thought it might be advantageous in ſome degree to the views of this public body to give the preſent account, ſhort and imperfect as it muſt neceſſarily be, and ſtill more to the world at large, if the exertions in favour of the ſciences in one part of the kingdom, ſhould, as is moſt probable, be followed by ſimilar proceedings in others.

On a new Kind of Mortar called Plaiſter Cement.

Account of a water cement.

Among the ſtones on the ſea-beach, near Boulogne, a particular kind is collected, which when calcined and pounded like plaiſter, forms a very hard cement with water. This ſubſtance has been uſed for œconomical purpoſes, and was found to poſſeſs the valuable quality of reſiſting water; under which fluid, it hardens very ſtrongly, and much more than in the air. Many conſtructions made with this cement afford the moſt complete proof of its ſolidity and tenacity. The detail of theſe experiments are given in a report preſented by *Le Sage* to the ſociety of Agriculture, Commerce and Arts, at Boulogne. Theſe muſt no doubt be of much local importance; but the analyſis of Guyton, as it tends to elucidate the compoſition of cements, is of more extended conſequence.

The ſpecific gravity was between 2,04 to 2,19 and 10 grammes, or about $\frac{1}{3}$ of an ounce produced in centegrammes.

Lime	403
Carbonic acid	330
Clay	187
Oxide of iron	70
Alumine	5
	995

The 187 centegrammes of clay afforded,

Silica	99
Alumine	39
Oxide of iron	43
	181

The ſtones are therefore compoſed of

Lime	403
Carbonic acid	330
Oxide of iron	113
Silica	99
Alumine	44
Loſs	11
	1000

Citizen Guyton ſhewed a veſſel to the ſociety d'Encouragement at Paris, which was very cloſe and firm in its texture, whence the abridger in the bulletin des Sciences, from which I take this article, expreſſes his opinion that it would prove very uſeful in the fabrication of various articles of pottery.

Note cônerning Two Brothers of a Race of Men reſembling Porcupines.

Men having ſcales and ſpines like porcupines.

Many philoſophers have already ſpoken concerning this race, the reality of which has been eſtabliſhed in a family well known by the name of Lambert. Two brothers of this family, all the males of which, have their bodies covered with ſpines or ſcales, are at preſent in Paris. One is 24 and the other 14 years of age. The body of the eldeſt is entirely covered, except the head and the inſide of the hands and feet: the youngeſt is naked in ſome places, particularly about the breaſt; but the brown ſpots on thoſe parts ſufficiently indicate that when he advances in age, he will become as rough as his brother. The ſpines on the back of the hand are very large, and may be compared for their diameter to the quills of the porcupine; but thoſe upon the breaſt have a greater reſemblance to ſcales: they are ſmall long plates, very numerous, and near together, being vertically implanted in the ſkin.

This thickening of the epidermis and hair, proceeds from a diſeaſe tranſmitted from generation to generation, but only from male to male. Five generations have been already afflicted with it.

Bull. de la Soc. Philomath.

Mawe's Mineralogy of Derbyſhire.

The Mineralogy of Derbyſhire, with a Deſcription of the moſt intereſting Mines in the North of England, in Scotland, and in Wales; and an Analyſis of Mr. Williams's Work, entitled, "The Mineral Kingdom." Subjoined is a Gloſſary of the Terms and Phraſes uſed by Miners in Derbyſhire. By JOHN MAWE, *London. Phillips, George Yard,* 1802, *octavo,* 211 *Pages, with* 4 *Engravings.*

The author of this uſeful book was employed by a Spaniſh gentleman to make ſurveys of our principal mines in Derbyſhire, to collect their various productions, and more particularly ſpecimens from each ſtratum, deſcribing their thickneſs, ſituation, and poſition; in order to ſhew an exact repreſentation of the mines for the royal cabinet at Madrid. To the obſervations thus made, he has added accounts of ſome mines in Scotland; a Tour from Glaſgow to Staffa; the ſalt mines at Northwich; the Parys Mine; Obſervations in Wales, and an Account of Mr. Williams's book, in 2 vols. called the Mineral Kingdom. This account ſeems too conciſe to be of great utility, and as a critique would have anſwered its purpoſe as well, if a few of the epithets had been more civil.

Speer on the hydrometer.

An Enquiry into the Cauſes of the Errors and Irregularities which take place in aſcertaining the Strength of Spirituous Liquors, by the Hydrometer, with a Demonſtration of the Practicability of ſimplifying and rendering this Inſtrument accurate. By WILLIAM SPEER, *Superviſor and Aſſayer of Spirits in the Port of Dublin. Octavo,* 48 *Pages. London,* 1802.

On the preſent occaſion, when the government of the country appears diſpoſed to ſettle this point of experimental philoſophy, upon which reſults of conſiderable importance to the revenue and to private property depend, but which have certainly been hitherto conducted in a ſlovenly and inaccurate manner, it muſt be very acceptable to all parties to ſee the ſubject clearly treated in a ſmall pamphlet. After a ſhort Introduction, the author gives an account of the origin and riſe of the hydrometer, the cauſes of its irregularities, the different modes of charging overproof; the principal defects in Clarke's hydrometer; the imperfection of the weights in that inſtrument; the general principles of floating inſtruments; the ſtandard of pure ſpirit; Gilpin's tables; modes of ſimplifying the hydrometer; deſcription of his own improved hydrometer: with anſwers to objections that probably might be made to it; and he concludes with a chapter on the neceſſity of a ſtandard for proof, and other practical objects of conſequence in this buſineſs.

Deſcription of Speer's hydrometer.—Stem octagonal; each ſide having the *per-centages* for different temperature.

M. Speer's own hydrometer conſiſts of a ball and ſtem, with counterpoiſe beneath as uſual, and the upper or graduated ſtem is made of an octagonal form. Upon each of the eight faces is engraved a ſcale of per-centages; by inſpection of which, the quality of the ſpirit is ſeen. But as the inſtrument will ſink to different depths in the ſame ſpirit, according to

its

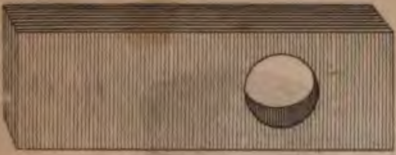
Speer on the Hydrometer.

its temperature, the ſcale upon each of the faces is adapted to a determinate temperature, namely 35°, 40°, 45°, &c. till the laſt, which is for 70°. The temperature of the ſpirit being therefore known, the reſult muſt be read, upon that face, at the top of the which the known temperature is engraved; and to prevent any miſtake, there is a ſmall index to be put on the ſtem to direct the eye of the obſerver to the proper face. And moreover, as the temperature is ſhewn only to every five degrees, there is another index of a different colour, which performs the office of a weight, and ſhews the intermediate temperature. This may alſo be effected by warming the ſpirits, by holding the glaſs in the hand till its temperature agrees with that marked on one of the faces; or, as I underſtand from the ſpecification of the author's patent, the preciſion of a ſingle degree, if required, may alſo be obtained, by four ſmall pins to be inſerted, one for each intermediate degree in holes, in the counterpoiſe below, where in fact they operate as weights of adjuſtment.

On the laſt day of this month, Mr. MACARTNEY will commence a COURSE OF LECTURES *in the* MEDICAL THEATRE OF SAINT BARTHOLOMEW'S HOSPITAL, *on the Anatomy of Animals and Vegetables,* from which will be deduced the general Doctrines of Phyſiology, or the laws of Organized Matter.

For Particulars, apply to Mr. NICHOLSON, at the *Apothecary's Shop, St. Bartholomew's Hoſpital.*

Sliding Stop-cock for pneumatic Experiments



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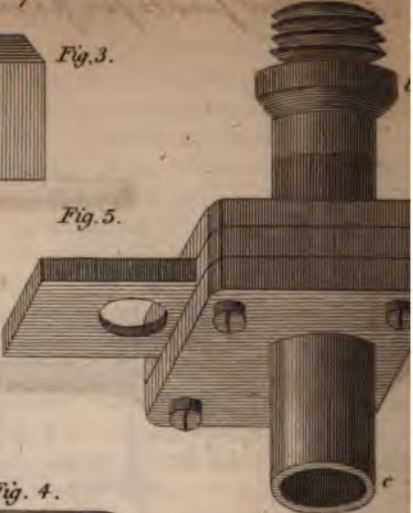

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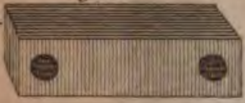

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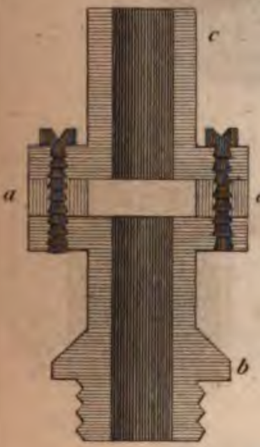

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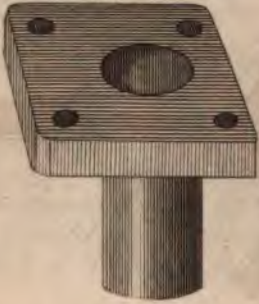

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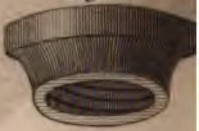

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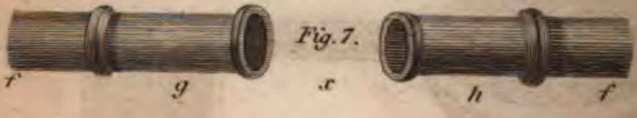

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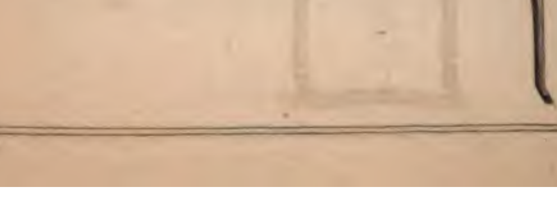

Fig. 8.

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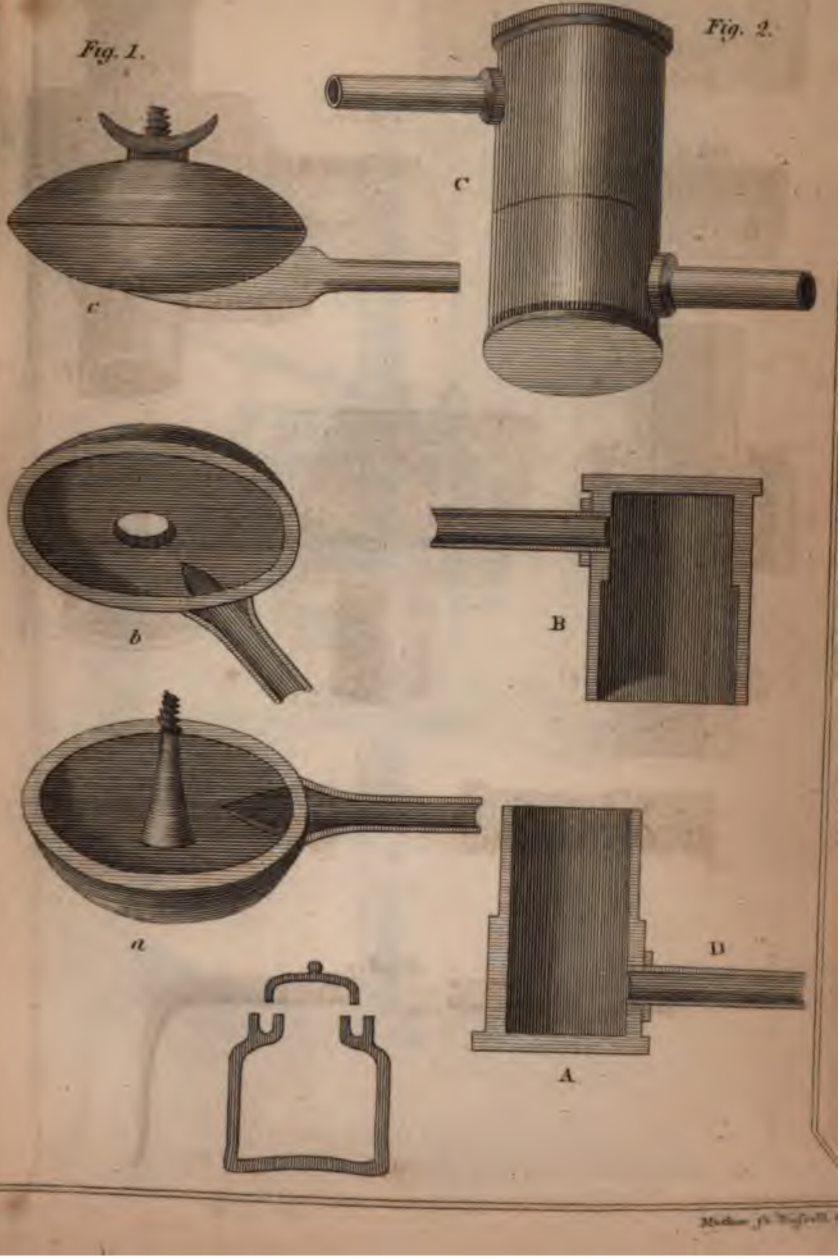
Metallic Joints for pneumatic Apparatus



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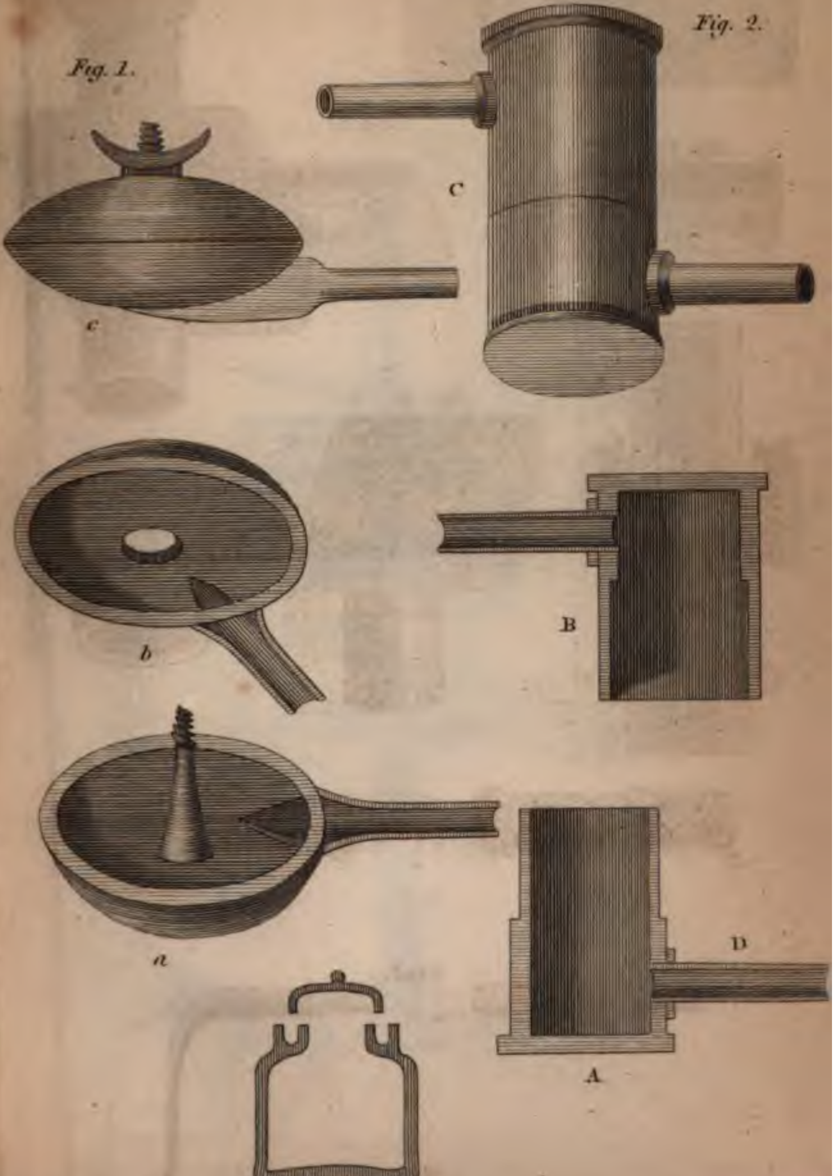

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Philos. Journal Vol. IV facing p. 64

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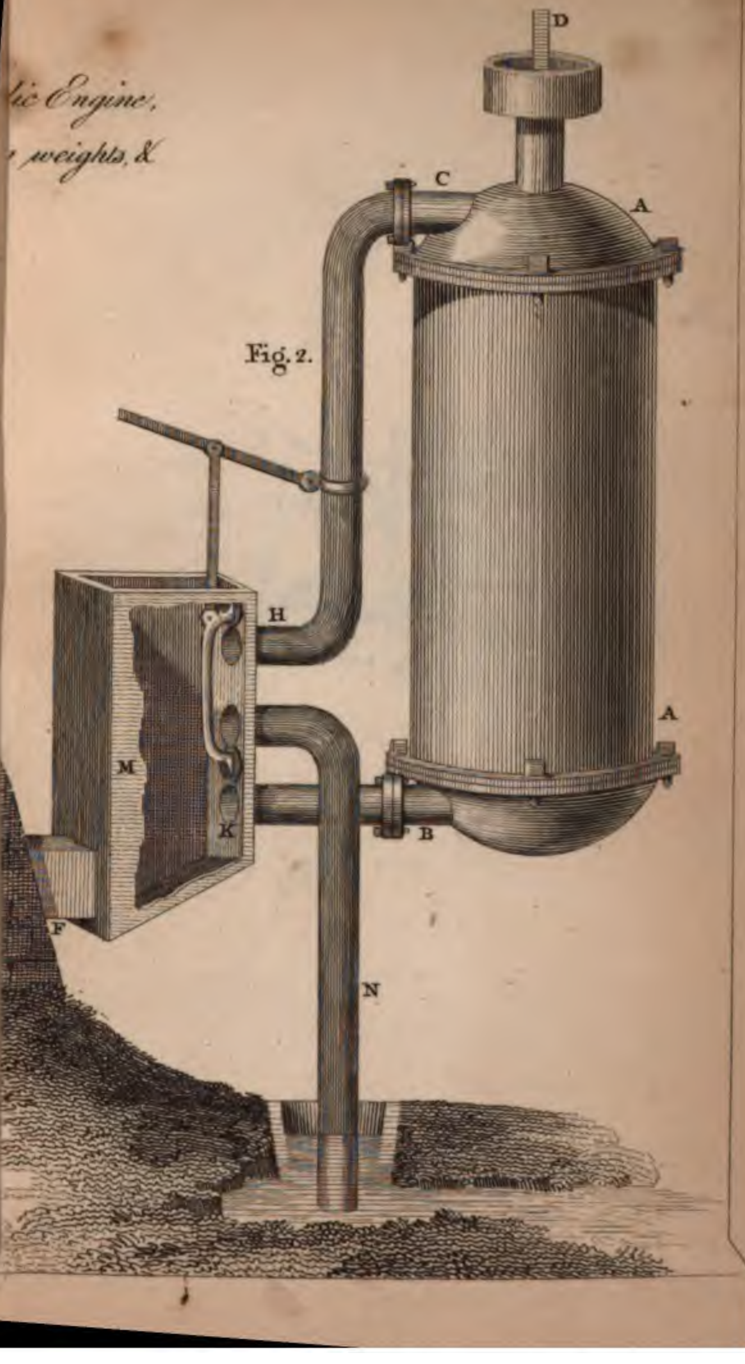
Fig. 2.



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

FEBRUARY, 1803.

ARTICLE I.

Anſwer to the Obſervations of Mr. WILLIAM CRUICKSHANK *upon the Doctrine of Phlogiſton* *. *In a Letter from the Rev.* JOSEPH PRIESTLEY, *L. L. D. F. R. S. &c.*

To Mr. NICHOLSON.

Northumberland, America, Nov. 5, 1802.

SIR,

Introductory paragraph.

YESTERDAY only I received the fifth Number of your valuable Journal, viz. for May, which contains Mr. Cruickſhank's reply to my defence of the doctrine of phlogiſton in anſwer to him; and it gives me ſincere pleaſure to obſerve, that he ſhews ſo much zeal in the defence of his hypotheſis, as by this means the truth will appear the ſooner. I only lament, that my great diſtance will neceſſarily make the controverſy of much longer continuance than it would otherwiſe be.

The quotation diſcuſſed by Mr. Cruickſhank ſtated to be of no conſequence to the argument.

Mr. Cruickſhank cenſures me for miſquoting his words, and miſtaking his meaning. If I have done ſo, I can aſſure him that it was without deſign; nor do I perceive that what he alledges of this kind is of any conſequence to the main argument. On the other hand, he ſays, p. 42, with an air of triumph, but before the victory, that "having additional difficulties to ſtruggle with, I have been under the neceſſity of adopting new, and ſometimes contradictory, opinions, in my explanations and defences."

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* Philoſophical Journal, II. 48.

The diſcuſſion attended with no difficulty.

Now I really am not ſenſible of any difficulty whatever in this diſcuſſion; and in advancing this he ſeems not to have apprehended my ſentiments with reſpect to the calces of metals. If he was acquainted with my publications, he would have found that there was nothing either new, or contradictory, in what I have now advanced on this ſubject, or that of the compoſition of charcoal; though that I have ſometimes changed my opinions on philoſophical as well as other ſubjects, I readily acknowledge, and without any feeling of ſhame.

Finery cinder contains water only. For (upon adding phlogiſton) water only is ſeparated.

In my opinion, and that of long ſtanding, the black calx of iron, commonly called *finery cinder*, contains no oxigen, but only water; and though the calx of zinc, and the yellow calx of lead, called *maſſicot*, do contain ſome, it is ſo little that I cannot detect it. Mr. Cruickſhank ſays, p. 42, that "if water be the only ſubſtance contained in oxides, heat alone ought at leaſt to revive ſome of them, and that in this caſe nothing but water would be ſeparated." Now what I maintain is, that when finery cinder is revived, (which, however, is not done without the introduction of phlogiſton) nothing but water is ſeparated from it.

The preſent letter is confined to that object.

Oxigen is undoubtedly contained in the red calx of lead, called *minium*, and in that of mercury; but I ſay that even theſe cannot be revived without the introduction of phlogiſton. This ſubject, however, I wave for the preſent, wiſhing to diſpatch that of the air from finery cinder and charcoal before we proceed to any other; and I wiſh Mr. C. to attend to the following obſervations, which I ſhall now ſtate more diſtinctly than I have done before, that it may be the eaſier for our readers to judge between us.

Mr. C's theory demands that oxigen ſhould quit iron to form fixed air with charcoal, and then return to the iron in order

1. Mr. Cruickſhank's hypotheſis requires that, in the proceſs of heating finery cinder and charcoal, the oxigen in the finery cinder ſhould quit that ſubſtance, and unite with carbon in the charcoal, in order to form fixed air. Since, however, this fixed air is to be decompoſed by iron, the oxigen which it

has got from the finery cinder muſt be ſeparated from it, and enter into the ſame calx again. But while the heat continues the ſame, I deem theſe contrary effects to be impoſſible. If the degree of heat that is applied expel oxigen from the calx, it will certainly prevent its return. Conſequently, if fixed air be formed, it cannot be decompoſed in theſe circumſtances.

to leave oxide o carbon; which cannot happen at the ſame heat.

2. If it was poſſible for oxigen to be ſeparated from finery cinder without any thing entering into it, which I think cannot be done, it could not, according to the new theory, form any thing by its union with carbon beſides *fixed air;* this being ſaid to be carbon diſſolved in oxigen gas, but nothing inflammable could be produced by their union. Of this Mr. Cruickſhank is ſenſible, and therefore he was under the neceſſity of ſuppoſing that, after the oxigen had quitted the finery cinder, it muſt enter into it again. But if this were poſſible, nothing would remain of the fixed air but *carbon,* as before that union; and this is a ſolid ſubſtance, incapable, without the aid of oxigen, of aſſuming the form of air. Whence, then, comes the inflammable air in this proceſs, which ſo nearly reſembles that from charcoal and water, that they muſt have the ſame origin; and with reſpect to *this,* Mr. Lavoiſier decides, that whatever is inflammable in it muſt come from water.

Carbon being conſidered as forming only on compound with oxigen (namely fixed air) it is concluded that no inflammable air could be had in reducing finery cinder; unleſs by mean of water.

3. Admitting all that Mr. Cruickſhank alledges concerning the difference in ſpecific gravity, and other circumſtances, between the air from finery cinder and charcoal, and that from water and charcoal, it is not ſo great as the difference between the latter and the light inflammable air from the metals with acids or water. Different as they may be in other reſpects, they are all inflammable, and have the common property of uniting with oxigen in a certain degree of heat; in conſequence of which they are the reverſe of *oxides,* and muſt be claſſed with *combuſtible* ſubſtances, equally with ſulphur and phoſphorus.

The inflammable gas, though different from that obtained by charcoal and water, is not a oxide but a combuſtible.

4. If the oxigen, after quitting the finery cinder, entered into it again, it would make it finery cinder as at the firſt, or at leaſt in ſome degree; whereas the calx is completely revived in this proceſs, the iron ſo revived being as ſoluble in acids as any iron whatever.

The oxigen doe not return to th finery cinder; for the iron is reduced and continues ſo.

5. If the iron ſhould be completely revived by the oxigen wholly leaving it, I ſtill maintain that it could not, by any

Iron cannot decompoſe fixed air. Mr. C's experiment leſ deciſive than that of the author.

degree of heat decompoſe fixed air. For my experiment with a *burning lens*, in which it could not be done, is far more unexceptionable than Mr. Cruickſhank's with *bladders and a gun barrel*. His objection to my proceſs has no weight. It was made with a few ounce meaſures of the air over mercury, with a lens ſixteen inches in diameter, and continued ſeveral hours, generally from ten o'clock to one; ſo that no particle of the air could eſcape being expoſed to a far greater degree of heat than could be communicated through a gun barrel. His experiment I have frequently made both in England and here, but could never be ſatisfied with the reſult. The ſcale upon the iron, I have no doubt, came from moiſture in the air, or from the bladders. Indeed, I cannot think that any perſon converſant as I have been with both theſe modes of operating, can heſitate in deciding that the preference muſt be given to mine.

Charcoal inferred to contain oxigen from Mr. Tenant's experiments.

6. Mr. Cruickſhank ſeems to think that charcoal cannot contain any oxigen; but Mr. Tenant's fine experiment deciſively proves that it does. For where are we to look for the oxigen (which we all acknowledge to be a component part of fixed air) which is ſeparated from the marble, but in the charcoal which is produced, and in that it makes part of a ſolid ſubſtance, and does not take the form of air.

If oxigen exiſted in finery cinder it ought to produce the phenomena of combuſtion in charcoal.

7. Since oxigen and all combuſtible ſubſtances unite, and explode, in a certain degree of heat, the oxigen that is expelled from the finery cinder uniting with carbon from the charcoal when red hot muſt enable it to *burn*; and therefore in theſe circumſtances there ought to be an exploſion, or at leaſt a gradual combuſtion of them in the courſe of the proceſs, as there is when oxigen is put to the ſame ſubſtance, and heated with it afterwards.

Invitation to Mr. Cruickſhank to enter into a full diſcuſſion of the new theory.

It is now near twenty years ſince this new theory was advanced, and from that time to the preſent I have not ceaſed to expreſs my opinion of its fallacy, and to give my reaſons for that opinion; but I have not till very lately been able to draw any degree of attention to the ſubject. Now, however, I am happy to have ſucceeded in this; and as I find that the chemiſts in France, the great patrons of the ſyſtem, look to Mr. Cruickſhank as the ableſt defender of it, I earneſtly wiſh that he would undertake the diſcuſſion of every article of my objections to it. What he has animadverted upon is only *one*

out

out of *eleven* articles in my *Tract on Phlogiſton;* and beſides this, he ſhould conſider what I have advanced on the ſame ſubject in other publications, eſpecially my experiments on the generation of air from water, both by evaporation and by freezing, and thoſe on the pile of Volta, and alſo ſeveral articles in the Tranſactions of the Philoſophical Society at Philadelphia.

As to the manner in which a controverſy of this kind is conducted, whether it be expreſſive of *reſpect*, or *contempt*, it will have little weight with a judicious reader. Preferring however a plain and calm diſcuſſion,

I am,
Dear SIR,
Your's ſincerely,
J. PRIESTLEY.

II.

The Conſtruction of an Apparatus for conducting Sound and holding Converſations at a Diſtance. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

SIR,

SOME of the moſt important inventions were at firſt made uſe of, either as playthings for children, or in exhibitions to amuſe the populace.

Several very important inventions were at firſt conſidered as trifling. Printing.

Printing, which has contributed more to the improvement of the human mind than any other art, was invented for amuſement, and the inſtruction of children *. But the true value of an invention is not immediately ſeen, it has to go through a long ſeries of improvements, before it arrives at a degree of perfection that is important to ſociety.

Effects of the loadſtone.

The loadſtone was long known to jugglers, and uſed by them in their exhibitions, before it was applied to the important purpoſe of navigation: and how far the improvements in mechanics, optics, and acouſtics, which are now exhibiting for amuſement, may hereafter become uſeful in the common affairs of men, is a matter on which we are not at preſent able to judge.

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* Hiſtory of Holland by Adrian Young.

Phantaſms by the camera obſcura behind a ſcreen.

The daggers and death-heads which are made to appear in the air, by means of a concave mirror; and the ghoſts and goblins, that are conjured up by means of the magic lantern of Philipſthal, not only ſerve as amuſement, but they may contribute to check the growth of ſuperſtition, by ſhewing in an agreeable manner how eaſy it is to impoſe upon the ſenſes.

Speaking machines.

Another way of deceiving the ſenſes, is by ſpeaking machines. Theſe are of very ancient date, and have been ſo much improved by the moderns, as to attract much attention.

One by Thomas Irſon in the 17th century.

No longer ago than the reign of Charles II. one Thomas Irſon excited much wonder in the king and his whole court, by a machine of this kind *. It is now well known that the ſound was conveyed to the mouth of the ſtatue by means of tubes artfully concealed; but the principle on which the ſpeaking machines are now conſtructed, appears to be at this time very little underſtood.

Sound conducted through timber.

It was known in the days of Pliny, that if a long beam of timber received a ſlight ſtroke at one end, the ſound was diſtinctly heard by a perſon whoſe ear was applied to the other end, though it could not be heard at the ſame diſtance through the air.

This property applied to a ſpeaking apparatus.

From the following experiments which I made on this property of wood, it appears that acouſtic inſtruments may be conſtructed for converſing at a diſtance, without the aſſiſtance of tubes to convey the ſound.

EXPERIMENT 1.

A deal rod 16 feet long was by contact made the medium between two trumpets.

I took a deal rod 16 feet long, and about an inch ſquare, and after having fixed one end of it into the ſmall end of a ſpeaking trumpet, I laid it upon two ſupporters or props in an horizontal poſition. One of the props was placed under the trumpet, about three inches from its wide end, and the other prop was placed near the other end of the rod. Another ſpeaking trumpet was then laid acroſs the rod, about three inches from the end. The wide end of this trumpet

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* Beckmann's Hiſtory of Inventions, III. 334.

reſted upon the rod, but the other end was ſuſpended by a ribband, from an object which need not be deſcribed.

After the apparatus had been prepared as above mentioned, I introduced my watch into the end trumpet, and on applying my ear to the croſs trumpet, I heard the beats much louder than if the watch had been at the diſtance of a few inches only. The ſound appeared to come out of the croſs trumpet, although the watch was at the diſtance of ſeventeen feet and a half: and when the watch was laid into the croſs trumpet, it was heard equally well at the end trumpet.

Sound was conveyed very diſtinctly through this apparatus.

It may not be improper to mention, that the ſound was increaſed by introducing a piece of metal, between the croſs trumpet and the rod upon which it reſted; but the manner of producing the beſt effect, I found by trial.

EXPERIMENT 2.

I placed another prop under the middle of the rod, and laid ſeveral books upon it in different places; but the rod conveyed the ſound juſt as well as before.

Bodies in contact with the rod did not prevent the effect.

EXPERIMENT 3.

My aſſiſtant in theſe experiments being ſeated at the end trumpet, and myſelf at the other, a converſation took place through this apparatus; but in whiſpers too low to be heard through the air at that diſtance. The converſation afforded us much entertainment; for when the ear was placed in a certain poſition, the words were heard as if they had been ſpoken by an inviſible being within the trumpet. And what rendered the deception ſtill more pleaſing, the words were more diſtinct, ſofter, and more muſical, than if they had been ſpoken through the air.

Converſation held by ſound paſſing through the rod both ways in a whiſper.

SPEAKING MACHINES.

What I have to ſay on this ſubject, muſt be ſo far anticipated by theſe experiments, as to render any particular deſcription of mine unneceſſary. It may not, however, be improper to mention my conjectures, reſpecting ſome uſeful purpoſes to which this acouſtic inſtrument may be applied. If a communication founded on theſe principles, were made between a ſhop and the dining room, or warehouſe, it might contribute to the diſpatch of buſineſs: and inſtruments made

Uſeful purpoſes to which theſe machines may be applied.

on the ſame principles might, perhaps, be found convenient in private houſes, if introduced between the parlour and ſome other room appropriated to the uſe of domeſtics. Directions might then be given to a ſervant without his entering the room, and in whiſpers too low to diſturb the company.

I am,
SIR,
your's reſpectfully,
EZEKIEL WALKER.

Lynn Regis, Jan. 19, 1803.

ERRATA.

Vol. IV. p. 40. in the firſt col. of the table, *for* 1, 2, 2, 4, *read* 1, 2, 3, 4.
P. 43. line 12, *for* long *read* large.
P. 43. line 19, *for*, In this manner, *read* Thus.

III.

Obſervations in Reply to Mr. GOUGH's *Letter on the Grave Harmonics**. *In a Letter from* THOMAS YOUNG, *M. D. F. R. S, &c.*

To Mr. NICHOLSON.

SIR,

It is the opinion of Lagrange that the grave harmonics are not the reſults of imagination.

AT length Mr. Gough has accepted my invitation, and has adverted to the phenomena of the grave harmonics. Theſe ſounds, he thinks, are merely mental and imaginary; I ſuppoſe them real and material; Lagrange, whom I have already quoted, is of the ſame opinion; and while I have the authority of a man who is allowed to be either the firſt or ſecond phyſical mathematician in Europe, I ſhall be very unwilling either to diſbelieve my ears, or to confeſs that Mr. Gough has convicted me of error.

Developement of the theory by that author.

To ſhew that I have not advanced a theory ſo new as Mr. Gough has deemed it, I ſhall quote a paſſage from the firſt volume of the Miſcellanea Taurinenſia. "We have ſeen," ſays Lagrange, "that the particle of air which is found in a place where two ſounds meet, receives an agitation different from that which is produced by each ſound; if therefore the ſounds are of ſuch a nature that their vibrations coincide always after a certain given time, the continued and regular impreſſion of theſe compound agitations may be diſtinguiſhed from the ſimple agitations, and an ear ſufficiently exerciſed will hear a third ſound, of which the relation to the others may be found by comparing the number of ſeparate vibrations that each of them completes between two ſucceſſive coincidences," p. 103. "And the compound agitation may be conveyed to the ear in an infinite number of —— ſituations." p. 104. "We have already ſpoken of the beats of Mr. Sauveur, and we have ſeen that they correſpond exactly with the coincidences of the vibrations; there is therefore every reaſon to believe that they are formed in the ſame manner by the meeting of two ſounds. And it is probable that the third ſound of Tartini is only produced by a ſeries of theſe beats." P. 105.

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* Philoſophical Journal, IV. page 1.

Were the conteſt to be decided by authority, it is probable that your readers would prefer that of Lagrange to Mr. Gough's and mine united: but we have no occaſion for any thing more than reaſon and experiment. If the mind were capable of making up a ſound in the way that Mr. Gough ſuppoſed, we ought to hear, whenever the impulſes of one ſound biſect either accurately or very nearly, the intervals between the impulſes of another ſound, an imaginary note, an octave above the ſeparate ſounds: if, on the contrary, my opinion is true, we muſt conclude that the retrograde motions of the one will counteract the direct motions of the other, and that both the ſounds will be deſtroyed.

If the grave harmonics be the reſult of compariſon; uniſons, of which the vibrations biſect each other, ought to give the octave;—but if they reſult from coaleſcence of undulations, the ſounds muſt here deſtroy each other.

Happily the point thus at iſſue may be determined by a very ſimple experiment: we have two ſounds ſtanding in this relation, in the intervals between the beats of two muſical chords tuned very nearly in uniſon. And if we liſten to a grave ſound which beats very ſlowly, while it is dying away, we ſhall obſerve, that in the interval of the laſt and fainteſt beats, when the ſound is leaſt mixed by reflections and irregular propagations, the note, inſtead of riſing to the octave, is wholly loſt.

Experiment to ſhew that the latter fact obtains in nature.

I confeſs with pleaſure, that Mr. Gough's explanation of the reſemblance which I have pointed out between the perception

The perception of a faint ſound from a tuning fork held between the teeth will be attended with a beat if another ſound nearly uniſon be tranſmitted through the air; whence it is inferred the ultimate direction of all ſounds is nearly the ſame.

ception of a grave harmonic, and the ſenſation of a ringing in the ears, is ingenious and probable; and I can mention a ſingular experiment in confirmation of his opinion. If a tuning fork, faintly vibrating, be held between the teeth, and a ſound nearly approaching to the ſame note be tranſmitted through the air, the beating will be nearly as diſtinct as if both ſounds arrived through the ſame medium. From this circumſtance, however, I only infer that all ſounds enter the ultimate organ of hearing nearly enough in the ſame direction to produce an alternate intenſion and remiſſion.

The grave harmonics differ from primitive ſounds; but they no leſs real than compound tides, &c.

I allow that ſuch a ſound differs from primitive ſounds in its want of appropriate direction, and in its mode of propagation. The daily tide at Batſha in Tunquin neither comes from the eaſt nor from the weſt, but it is as much a real tide as the grave harmonics are real ſounds. If any perſon ſhould inſiſt that the phenomenon is not a tide, but an alternate elevation and depreſſion of the water only, and that it exiſts only in the ſenſations of the obſervers, and not in the ſea, I ſhould be very little diſpoſed to enter into arguments with him on the ſubject.

Remarks on the proper vehicles of philoſophical papers. Philoſ. Tranſ.—Provincial Memoirs. Journals.

With reſpect to the communication of my remarks to the Mancheſter Society, I beg leave to reply, that if I thought a paper of permanent importance to the extenſion of ſcience, I ſhould conſider myſelf as bound by my duty to the Royal Society and to poſterity, to offer it for inſertion in the Philoſophical Tranſactions; but if it were of a mere ſuperficial and temporary nature, I ſhould think it ſufficient to publiſh it in a reſpectable literary Journal. I do not mean any diſreſpect to provincial ſocieties, but many papers have appeared in the volumes of the Mancheſter Memoirs, which would perhaps have been more ſuitably placed in thoſe of the Philoſophical Tranſactions.

Your obedient,
humble ſervant,
THOMAS YOUNG.

Jan. 10, 1803.

IV. *Experiments*

IV.

Experiments and Obſervations on the Power of Fluids to conduct Heat; with Reference to Count Rumford's Seventh Eſſay on the ſame Subject. By JOHN DALTON.

(*Concluded from Page* 58.)

Short recapitulation of Count Rumford's experiment of ice at the bottom of heated water.

COUNT Rumford's principal experiments are thoſe in which a cake of ice was confined on the bottom of a cylindrical glaſs jar, of 4.7 inches in diameter, and 14 high, and water poured upon it of different temperatures ſuffering it to ſtand, without agitation. He found that about 6lb of boiling hot water melted little more ice than as much water of 41°; and that by making ſuch allowances as the experiments ſeemed to warrant for deductions when hot water was uſed, water of 41°, or 9° above the freezing point, melted quite as much, and often more, than the hot water: From which he infers, that water. and by analogy all other fluids, do not tranſmit heat in the manner that ſolids do, but circulate it ſolely by the internal motion of their particles.

The internal motion or circulation.

The exiſtence of this internal motion he has proved decidedly; that water of a certain temperature being of the greateſt denſity, will always take the *loweſt* place, and water either warmer or colder than that degree will aſcend. This degree of greateſt condenſation he takes on the authority of others at 40°; it appears however from the experiment related above, to be ſtill more favourable to his poſition, namely 42½°: and that water of 32° muſt aſcend till it comes to water of 53°, if it be not cooled in its progreſs, which circumſtance he admits.

Conſideration of the facts:

Upon conſidering the facts related in his experiments therefore, there are *three* cauſes which ſuggeſt themſelves as conſpiring to circulate and diffuſe the heat, by which the ice is melted.

1. Cold water at 32° riſes to the place of 53°, and warmer of 42½° deſcends.

1ſt. The internal motion of the liquid, by which water of 32°, incumbent upon the ice, is perpetually aſcending into a warmer region of 53°, and warmer water of 42½° deſcending to take its place.

2. The fluid itſelf may conduct.

2d. The proper conducting power of the liquid independent of internal motion.

3. The glaſs itſelf may conduct; probably little.

3d. The conducting power of the glaſs jar. But as glaſs is known to be a very bad conductor of heat, it can produce no material effect in theſe experiments: for which reaſon Count Rumford does not appreciate the third cauſe.

Count Rumford does not allow that a cold fluid riſing into a warmer can cool it;

With reſpect to the operation of the firſt cauſe, it will *generally* be ſuppoſed that *cold* water riſing into *warmer* and remaining with it, the heat is impaired, and the two reduced to a common temperature. But Count Rumford does not admit of this communication; he maintains, that the two ſtill retain their proper ſhare of heat, notwithſtanding they are mixed together. This hypotheſis of his is of no peculiar conſequence as far as reſpects the effect of the internal motion: for the temperature indicated by a thermometer immerſed in an equal mixture of water at 32° and 53°, would be the ſame as if the water was uniformly of the temperature 42½°. But it has material conſequences in other reſpects; for, if it be admitted, it annihilates the *ſecond* cauſe abovementioned, and it would follow that warm water being put upon cold water above the temperature of 42½°, the heat could not in any degree be propagated downwards, unleſs by agitation, and even then, upon ſubſiding, the warm part ought to riſe to the top, and the cold fall to the bottom.

whence it would follow that heat cannot by tranſmiſſion downwards throw water above 42½°.

He has not proved this by experiment.

Theſe poſitions are ſo manifeſtly contradictory to common opinion, that they cannot be received without proof. But Count Rumford has not given us a ſingle experiment to prove them. It ſeemed neceſſary therefore, to clear up this point by direct experiments.

Experiment 1.

Examination by Exper. 1. Hot water poured upon cold air in a glaſs veſſel, the upper part gradually cooled, and the lower heated.

Took a large tumbler glaſs, 3½ inches diameter, and five inches deep, and filled it half way with water of 51°, then gently filled up the reſt by means of a ſmall ſyphon, with water of 88°; a thermometer, with its bulb and ſtem detached from the frame, being previouſly immerſed to the bottom. The temperatures at the top and middle were had by gently immerſing the bulb of another thermometer into the water.

Time elapſed.	TEMPERATURE *at top.*	*in the middle.*	*at bottom.*
——	88°	——	51°
5 min.	85	——	54
12	83	75°	56
18	80	72 +	58
30	76	——	60
40	73	——	61
50	70	——	61
60	69	——	61

(Air in the room 50°.)

Experiment 2.

The ſame as before; only a circular piece of wood floated upon the ſurface of the water, on the centre of which the ſtream of the ſyphon was directed to prevent the current downwards.

Exp. 2. The ſame with more precaution.

(Air in the room 55°.)

Time.	TEMPERATURE. *at top.*	*at bottom.*
Before the water was poured on		56°
—	116°	56 +
10 min.	105	56½
20	92	57 —
30	85	57⅓
40	80	57¼
50	77	58 +
1 h. —	75	58½
— 10	72½	58½
— 20	70	59 —
— 30	67	58½
— 40	65	58 +
— 50	63	58
2 —	62	58 —
— 15	61	57½
— 30	59½	57
3 —	57½	56 (Air 52°)
5 —	53½	53 Do.

A ſimilar reſult was obtained in a different way by the following.

Experiment

Experiment 3.

Exp. 3. The upper part of water heated by red hot iron; the lower part became alſo hotter.

Took an ale glaſs of a conical figure, 2½ inches in diameter and 3 inches deep; filled it with water that had been ſtanding in the room, and conſequently of the temperature of the air nearly—Put the bulb of a thermometer to the bottom of the glaſs, the ſcale being out of the water: Then, having marked the temperature, I put the red hot tip of a poker, half an inch deep into the water, holding it there ſteadily about half a minute; and as ſoon as it was withdrawn, I dipt the bulb of a ſenſible thermometer into the water about ¼ inch, when it roſe in a few ſeconds to 180°.

	TEMPERATURE		
Time.	*at top.*	*middle.*	*bottom.*
Before the poker was immerſed			47°
—	180°	—	47
5 min.	100	60°	47½
20	70	60	49
1 h. —	55	—	52

Theſe experiments all evidently agree in proving water to have a proper conducting power, independent of any internal motion. It ſurely will not be ſaid that any ſlight motion unavoidably made at the beginning of an experiment, could continue with a powerful effect for upwards of an hour. However, to determine this matter, I made the two following experiments.

Experiment 4.

Exp. 4. Coloured water was heated by hot colourleſs water reſting upon it, without mixture.

Took the glaſs tumbler of the firſt experiment, and filled it half way with rain water, deeply tinged with archil; then filled it up with clear warm water, as related in the 2d experiment. The upper half was but juſt perceptibly tinged by the proceſs and uniformly ſo; it remained for an hour not viſibly altered in this reſpect, though by frequently putting the bulb of a thermometer down to the middle, the colour at laſt roſe in a ſmall degree.

(Air 45°)
TEMPERATURE

	at top.	*middle.*	*bottom.*
Before the warm water was poured on			44°
Time	105°	—	—
7 min.	97	—	47+
17	86	—	48
27	79	—	49+
37	75	68	50
47	70	66	50+
57	66	62	51½
1 h. 7	60	62	51½
— 17	60+	59	51½
— 27	59	—	51½

Experiment 5.

A glaſs tube near an inch in diameter, and 16 inches long, was half filled with a coloured ſolution of common ſalt in water, warm; a ſmall thermometer was wholly immerſed in it, and cold clear water carefully poured upon the whole ſo as to fill the tube; the colour aſcended very little, and continued invariable after the proceſs of filling. The warm ſolution was of courſe made of greater ſpecific gravity than the cold water.

Warm coloured ſalt and water was cooled by cold freſh water reſting on it without mixture.

(Air 45°)
TEMPERATURE

	at top.	*bottom.*
Time.	45°	85°
5 min.	53	79
10	53	74
21	52	69
31	51	66
45	50½	64
58	50	61
1 h. 31	49	56½
3 30	47	51
— 55	47	50
4 15	—	49
7 5	46	48

To determine whether hot and cold water being ſuddenly mixed, and agitated, the hot would afterwards riſe to the top, was the object of

Experiment 6.

Exp. 6. Hot and cold water being mixed, did not afterwards ſeparate.

Air in the room 50°.—About ½ pint of water of 130° was poured into a cold tumbler glaſs, and immediately after as much water of 50°; the mixture was agitated for half a minute by a deal rod; after which an immerſed thermometer ſtood at 85°, both at top and bottom; it was then ſet by in a ſtill-place for examination.

	TEMPERATURE	
Time.	*at top.*	*bottom.*
15 min.	77¼°	77°
30	73	72⅔
45	68	67⅔
1 h: —	64.8	64.6

From all which water is concluded to be a proper conductor.

From all theſe experiments it is evident, that water has a proper conducting power: In the laſt experiment, if the particles of water during the agitation had not actually communicated their heat, the hot ones ought to have riſen to the top, and the cold ones ſubſided ſo as to have made a material difference in the temperature. It is, however, equally evident, that water is a *bad conductor* of heat, probably as it is of electricity; the deſcent of the heat in the ſecond experiment is wonderfully ſlow; a ſlight agitation for one *ſecond* would do as much to induce the equilibrium as ſtanding ſtill one *hour*. In repeating the third experiment, in a wine glaſs, I have ſeveral times known water ½ an inch deeper to differ 50° in temperature from the incumbent water.

We muſt conclude, therefore, that the quick circulation of heat in water over a fire, &c. is owing *principally* to the internal motion excited by an alteration of ſpecific gravity; but not *ſolely* to that cauſe as Count Rumford has inferred.

If it be proved that water conducts heat, it will ſcarcely be neceſſary to prove, that other fluids conduct it, and that they communicate it one to another:—The two following experiments ſhew that mercury conducts it, and that water and mercury reciprocally communicate it.

Experiment

Experiment 7.

Took a cylindrical glaſs tube, of 1 inch internal diameter, and put 1¾ inches in depth of mercury into it, and immerſed the bulb and ſtem of a thermometer to the bottom, the ſcale as uſual being above the liquid; then put 2¼ inches of warm water upon it by a ſyphon, and let it ſtand without agitation.

Exp. 7. Hot water, reſting on mercury, heated it.

TEMPERATURE.

	Merc.	*Water.*
Time.	56°	122
3 m.	70	118
6	73	110
11	75	100
14 m.	74½°.	92°.
19	73	87
27	71	78

Experiment 8.

Into a tumbler glaſs, 2½ inches in diameter, poured an inch in depth of mercury, and heated it to 110°; upon which was poured an inch of water at 50°, and then kept ſtill.

Exp. 8. Cold water upon hot mercury, was heated.

TEMPERATURE.

	Merc.	*Water.*
Time.	110°	50°
4 min.	74	70
8	71	70½
10	70½	70

Finding that water was ſo bad a conductor of heat, I was deſirous to learn how ice would conduct it, and tried it as follows.

Experiment 9.

Feb. 9th. Out of a maſs of ice, by means of a hot iron, I ſhaped a cylindrical piece, 3 inches in diameter, and 5½ inches long, clean and pure; its weight 17 ounces. Made a ſmall round hole at one end, one inch deep, and the ſize of a thermometer bulb, which was incloſed in it. The other end of the piece was put into a baſon of ſnow and ſalt, to the depth of from ½ to 1½ inches, the temperature of which was kept below 10° for 1¾ hours. Air 37°.

Ice, immerſed partially in the freezing mixture, conducted very badly,—much worſe than water.

Time elapſed.	Therm. in the liquid.	Therm. in the ice.
	5°	32°
1¾ h. at a medium	7	31½

N. B. This deſcent of half a degree was gradual, but did not commence till long after the beginning of the experiment. After this the piece of ice was inclined to one ſide, by which nearly one half of it was immerſed in the cooling liquid, and the incloſed bulb of the thermometer was now not more than an inch from the cold mixture.

H.	M.	Therm. in the liquid.	Therm. in the ice.
1	50*	14°	28°
2	20	19	28
2	50	22	Ice along with the therm. ſlipped down into the cold liquid.

The ice now weighed 12½ ounces: the reſt had been liquified by the operation of the ſaline liquor.

This experiment, I think, decidedly proves that ice is a worſe conductor of heat than water: indeed this is not wonderful; for it is ſaid, that ice at a low temperature becomes an electric.

Mixture is much more effectual for equalizing temperature, becauſe hot and cold particles are brought together.

It is certainly a remarkable circumſtance, but not at all inconſiſtent with the known laws of heat, that in a mixture of hot and cold liquids, the uniform temperature ſhould be ſo ſoon induced by agitation and ſo ſlowly by reſt: but when we conſider, that in the former caſe, hot and cold particles are brought together, and that in the latter there is a ſeries of particles one upon another, gradually riſing in temperature, but differing by inſenſible degrees, we ſhall not wonder at the facts. When any one particle of water, or any other body, has one above it, warmer by an inſenſible degree, and another below it, colder by an inſenſible degree, its power to tranſmit heat muſt be very ſmall †. Theſe conſiderations gave riſe to the two following experiments.

The gradual difference of temperature renders water a bad conductor.

Experiment 10.

Exp. 10. An heated mercurial thermometer expoſed to air, gave out moſt heat when at the higheſt temperatures.

A mercurial thermometer was taken, its bulb $\frac{4}{10}$ inch in diameter, and hanging clear of the ſcale: it was heated by the flame of a candle to 600°, and then laid upon a table with the bulb projecting over the edge, and was thus left to cool by the mere operation of the air in the room, which was 52°. The following is the medium reſult of two experiments, which, however, agreed with each other almoſt in every obſervation.

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* From the beginning of the experiment.

† Will not this argument apply to ſolids univerſally? contrary to fact. N.

Time.	*Temp.*	*Time.*	*Temp.*
0	600°	18 half m.	66°
1 half m.	450	19	64
2	350	20	62
3	280	21	60
4	229	22	59
5	195	23	58
6	168	24	57
7	145	25	56
8	128	26	55
9	115	27	54
10	104	28	53+
11	95	29	53
12	88	30	53
13	81	31	53
14	77	32	52+
15	73	33	52
16	69+	Air in the room	52
17	68		

Experiment 11.

Another thermometer, having a ſimilar bulb, but a ſcale with much larger degrees, was heated and cooled in the ſame manner.

Time.	*Temp.*	*Time.*	*Temp.*
—	85°	16	56.3°
1 half m.	79½	17	56¼
2	75	18	56
3	71+	19	55.9
4	68½	20	55.7+
5	66½	21	55.6+
6	64½	22	55.5
7	63	23	55.4
8	61½	24	55.3
9	60	25	55.25
10	59	26	55.2—
11	58¼	27	55.1+
12	58	28	55.1—
13	57¼	29	55.+
14	57	30	55
15	56.6		

In theſe experiments we may conſider mercury and air mixed together of unequal temperatures, with a thin partition of glaſs—and from the laſt we may conclude, that the thermometer imparted to the air 40 times more heat in half a minute, when its temperature was 30° above the air, than when it was only 1° above it.

Argument from the Count's own experiments. No water but that below 53° could deſcend to the ice, and conſequently this only would be cooled, and the fuſion of the ice would then ceaſe.—Contrary to fact.

We ſhall now advert a little to Count Rumford's experiments. It will eaſily appear, that arguing fairly upon his own hypotheſis he can never account for the phenomena obſerved: for, hot water being poured upon ice, an internal motion would take place near the ſurface of the ice, by which a ſtratum of water of a certain thickneſs would be reduced to 32°, and then all further reduction of the ice muſt ceaſe; becauſe all the ſuperincumbent water being above 53° would be lighter and could not deſcend to the ice. But this is quite contrary to what took place. The facts, however, will admit of a ſatisfactory explanation upon eſtabliſhed principles.

Explanation from the known facts. The ice is fuſed (1) by the proper conducting power of the water; and (2) by its motion. The firſt is as the heat, and the ſecond diminiſhes in ſome unknown ratio as the temper. differs from 42½.°

By experiments 10 and 11, it appears, that the quantity of heat given out by a body, during any ſmall given portion of time, is nearly as the exceſs of the temperature of the body above the cooling medium. Hence, then, we may conclude, that the effect of hot water upon ice ariſing from the proper conducting power of water, will be nearly as the heat of the water. What effect the other cauſe may produce, it will be difficult to determine from theory: experience will be the beſt guide. One thing, however, appears pretty certain, that its effect muſt be a *maximum*, when the temperature of the water at large is 42½°; becauſe then there can never want a determination of the particles *downward* to ſupply the place of the lighter water of 32° aſcending. If the temperature of the water exceed 42½°, then the effect of the internal motion will be leſs, diminiſhing by ſome unknown ratio. As far as I can judge from Count R's experiments, the joint effects of thoſe two cauſes ſhould be nearly the ſame with water of 42° and water of 190°. Taking this, therefore, for granted, we ſhall be enabled to ſketch a table of the values of theſe two cauſes for every 10° of temperature. The numbers expreſſing the effect of the proper conducting power, are derived from the 10th experiment, and conſequently are not purely hypothetical: thoſe expreſſing the other effect, except 42° and 192°, are put down hypothetically, becauſe the law of decreaſe has not been aſcertained.

Water at 42½° appears to fuſe (chiefly by its motion) nearly the ſame as water at 190° (chiefly by its heat.)

The reſults ſketched out for different temperatures.

It is to be ſuppoſed, that a given quantity of water, of the ſeveral temperatures mentioned, is carefully poured upon a cake of ice at the bottom of a cylindrical glaſs jar, and ſtands without agitation for a given time, as half an hour; then the proportionate quantity of ice ſuppoſed to be melted by the two cauſes ſeparately are ſtated in numbers, and then the ſums are taken to expreſs the joint effects.

Temperature of the water on the ice.	32.°	42.°	52.°	62.°	72.°	82.°	92.°	102.°	112.°	122.°	132.°	142.°	152.°	162.°	172.°	182.°	192.°	202.°	212.°
Ice melted by the conducting power of the water.	0.	2.	3.	6.	9.	11.	12.	14.	17.	19.	21.	23.	25.	27.	30.	33.	35.	38.	40.
Ice melted by the internal motion of the particles of water.	0.	36.	35.	31.	25.	19.	16.	14.	12.	10.	8.	7.	6.	5.	4.	3.	3.	3.	3.
Total quantity melted by both cauſes.	0.	38.	38.	37.	34.	30.	28.	28.	29.	29.	29.	30.	31.	32.	34.	36.	38.	41.	43.

After what has been ſaid, I need not caution my readers not to conſider this table as accurate. The principle of it, however, cannot I conceive be diſproved: that the operation of the conducting power muſt be proportionate to a ſeries of numbers beginning from 0 at 32°, and gradually increaſing in ſome ratio with the temperature above 32°, cannot, I think, be controverted; and that the operation of the internal motion muſt begin from 0 at 32°, and increaſe till it arrives at its *maximum* at 42½°, and then decreaſe again ever after, is alſo, I apprehend, unqueſtionable: thus, when the jar had water of 42°, in Count R's experiments, this internal motion muſt have had a range of 8 inches in depth; whereas, when hot water alone was uſed, it had not more than ¼ of an inch to range from the temperature of 32 to that of 53°.

Obſervations on the table.

The following table exhibits a conciſe view of all the material varieties of Count Rumford's experiments, with their reſults.

Table of the varieties of the Count's experiments.

Experiment

	Temperature of the water when poured on the ice.		Medium Temperature.	Water in the jar surrounded by		Ice melted in 30 minutes.
	At the beginning	end				
Experiment 32	41°	40°	40½°	Air	41°	617 Grains
35	189	180	184½	*a warm covering of cotton wool.*		747
38	41	43	42	Air	61	676
39	180	157	171½	Air	61	559
45	188	64	128	Ice & water	32	406
51	61	49	55	Ice & water	32	660
53	61	60	60½	Air	61	642

Remarks.

Count Rumford attempts to explain why there was leſs ice melted in ſuch experiments as the 45th than in thoſe like the 39th, and attributes the diminution of the effect to the deſcending currents, occaſioned by the cold mixture ſurrounding the warm one, which he thinks would obſtruct the oppoſite ones aſcending from the ice. But the effect in the 51ſt, compared with the 53d, being juſt oppoſite, he paſſes over without explanation.—I have no doubt myſelf, but that the true cauſe of the differences in both caſes, is to be found in the column expreſſing the *mean temperature* of the water, and not in that expreſſing its ſituation, which I conſider as having nothing to do in the buſineſs, but as it affects the general temperature. The *maximum* effect with cold water will be when it is of the temperature of about 48° or 50°, and the *minimum* above it probably about 100° or 120°; and in proportion as the mean temperatures, in any experiment, deviate from thoſe points, the effects vary accordingly, let other circumſtances be what they may.

The differences depend on the mean temperature of the water.

Concluſion.

Thus I have attempted to explain the rationale of theſe very curious and intereſting experiments, in a manner different to what their ingenious author has done. And muſt now leave it to the reader to form his opinion.

V.

A Method of examining refractive and diſperſive Powers, by priſmatic Reflection. By WILLIAM HYDE WOLLASTON, *M. D. F. R. S.* *

New method of examining the refracting and diſperſing powers of bodies, upon light;

IN examining the power with which various ſubſtances refract and diſperſe light, I have for ſome time paſt employed a method unnoticed by writers on optical ſubjects; and, as it is not only convenient in common caſes of refraction, but alſo capable of affording reſults not attainable by other means, I have been induced to draw up a ſhort account of the method itſelf, and of the moſt remarkable inſtances of its application.

This method was ſuggeſted by a conſideration of Sir Iſaac Newton's priſmatic eye-glaſs, the principle of which depends on the reflection of light at the inner ſurface of a denſe refracting medium.

by angle of total reflection at the

Since the range of inclination within which total reflection

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* Philoſ. Tranſ. 1802.

takes

confine of the denſer of two mediums:

takes place, depends not only on the denſity of the reflecting priſm, but alſo on the rarity of the medium adjacent to it, the extent of that range varies with the difference of the denſities of the two media. When, therefore, the refractive power of one medium is known, that of any rarer medium may be learned, by examining at what angle a ray of light will be reflected from it.

General fact ſtated. The angle is greater, the greater the refractive denſity of the medium towards which the light paſſes;

For inſtance, when any object is laid under a priſm of flint-glaſs, with air alone interpoſed, the internal angle of incidence at which the viſual ray begins to be totally reflected, and at which the object ceaſes to be ſeen by refraction, is about 39° 10′; but, when the object has been dipped in water, and brought into contact with the glaſs, it continues viſible, by means of the higher refractive power of the water, as far as 57½° of incidence. When any kind of oil, or any reſinous cement, is interpoſed, this angle is ſtill greater, according to the refractive power of the medium employed; and, by cements that refract more ſtrongly than the glaſs, the object may be ſeen through the priſm, at whatever angle of incidence it is viewed.

until total tranſmiſſion.

Fluids are eaſily applied to the denſe medium (or glaſs): But ſolids require the interpoſition of a fluid more refringent than themſelves.

In examining the refractive powers of fluids, or of fuſible ſubſtances, the requiſite contact is eaſily obtained; but, with ſolids, which can in few inſtances be made to touch to any great extent, this cannot be effected without the interpoſition of ſome fluid, or cement, of higher refractive power than the medium under examination. Since the ſurfaces of a ſtratum ſo interpoſed are parallel, it will not affect the total deviation of a ray paſſing through it, and may therefore be employed without riſk of any error in conſequence.

Thus, reſin, or oil of ſaſſafras, interpoſed between plate glaſs and any other priſm, will not alter the reſult.

A triangular glaſs priſm may be uſed for comparing the powers of two bodies;

If, on the ſame priſm, a piece of ſelenite and another of plate-glaſs be cemented near each other, their powers may be compared with the ſame accuracy as if they were both in abſolute contact with it.

but a ſquare priſm ſhews the ſine of refraction without calculation.

For ſuch a mere compariſon of any two bodies, a common triangular priſm is beſt adapted; but, for the purpoſe of actual meaſurement of refractive powers, I have preferred the uſe of a ſquare priſm, becauſe, with a very ſimple apparatus, it ſhows the ſine of refractive power ſought, without the need of any calculation.

Figure and application of the *ſquare priſm.*

Let A, Fig. 1, Plate IV, be a ſquare or rectangular priſm, to which any ſubſtance is applied at *b*, and let any ray of light parallel to *c b* be refracted through the priſm, in the direction *b d e*.

Conſtruction to ſhew the ſine of refraction from any given rarer body into glaſs.

Then, if *e f* and *e d* be taken proportional to the ſines that repreſent the refractive powers of the priſm and of air, *f g*, which is intercepted between *f* and the perpendicular *e g*, will be the correſponding ſine to repreſent the refractive power of the medium *b*. For, ſince *e d g* (oppoſite to *e f*) is the angle of refraction, *e f g* (oppoſite to *e d*) muſt be equal to the angle of incidence *b d h*; and *e f* : *f g* : : *b d* : *d h* : : ſine of *c b i* : ſine of *h b d*.

Inſtrument giving the ſine by inſpection.

All therefore that is requiſite for determining the refractive power of *h*, is to find means of meaſuring the line *f g*. On this principle, the inſtrument in the annexed ſketch (Fig. 2.) is conſtructed. On a board *a b* is fixed a piece of flat deal *c d*, to which, by a hinge at *d*, is jointed a ſecond piece *d e*, 10 inches long, carrying two plane ſights at its extremities. At *e* is a ſecond hinge, connecting *e f*, 15,83 inches long; and a third at the other extremity of *e f*, by which *f g* is connected with it. At *i* alſo is a hinge, uniting the radius *i g* to the middle of *e f*; and then, ſince *g* moves in a ſemicircle *e g f*, a line joining *e* and *g* would be perpendicular to *f g*.

The piece *c d* has a cavity in the middle of it, ſo that, when any ſubſtance is applied to the middle of the priſm P, it may continue to reſt horizontally on its extremities. When *e d* has been ſo elevated that the yellow rays in the fringe of colours (obſervable where perfect reflection terminates) are ſeen through the ſights, the point *g*, by means of a vernier which it carries, ſhows by inſpection the length of the ſine of refraction ſought.

This method is moſt accurate and eaſy: it requires only one ſurface and gives the reſult at ſight.

The advantages which this method poſſeſſes above the uſual mode of examining refractive powers, are greater than they may at firſt ſight appear. The uſual practice has been, to form two ſurfaces of the ſubſtance under examination, ſo inclined to each other that the deviation occaſioned by them might be meaſured. The inclination of theſe ſurfaces to each other muſt alſo be known; and thence the refractive power might be computed. But, in the method here propoſed, it is ſufficient to have only one ſurface, and the reſult is obtained at once, without computation.

Its facility renders it uſeful as a teſt in philoſophical inquiries.

The facility of determining refractive powers, is conſequently ſuch as to render this property of bodies a very convenient teſt in many philoſophical inquiries. For diſcovering the purity of eſſential oils, ſuch an examination may be of conſiderable utility, on account of the ſmallneſs of the quantity requiſite for trial. In oil of cloves, for inſtance, I have met with a wide difference. The refractive power of genuine oil of cloves, is as high as 1,535; but I have alſo purchaſed oil by this name, which did not exceed 1,498, and which had probably been adulterated by ſome leſs refractive oil.

It is applicable to opake bodies.

For ſuch purpoſes, the refractive power of opaque ſubſtances may often be deſerving of inquiry, which could not be learned by any means at preſent in uſe. For, in the uſual mode, a certain degree of tranſparency is abſolutely neceſſary; but, for trial by contact, the moſt perfect opacity does not occaſion the leaſt impediment.

Inſtance.

Among other inſtances in which I have taken advantage of this circumſtance, I may mention a ſubſtance that had been found in one of the iſlands of the North Pacific Ocean, which, to all outward appearance and by various trials, ſeemed to be perfect bees-wax, although it is ſuppoſed that there are no bees in the iſland from which it was brought. On placing it by the ſide of a piece of bees-wax, in contact with a priſm, the perfect equality of their refractive powers afforded a ſtrong confirmation of the opinion before formed of their identity.

Bodies of varying denſity may be examined by it,

For the examination alſo of media of which the refractive denſity is not uniform, the general method of trial by deviation wholly fails; on the contrary, by placing a varied medium in contact with a priſm, all its gradations of denſity, from greateſt to leaſt, become at once the object of mere inſpection. An inſtance of this may very readily be ſeen with a piece of gum, the ſurface of which has been moiſtened for a few minutes; when, by cloſe application to a priſm, a refractive power may be diſcerned, varying from that of the water on the ſurface, 1,336, to nearly 1,51, the refractive power of gum arabic.

particularly the cryſtalline lens.

I ſhould not ſo much inſiſt on this advantage, were it not for the opportunity hereby afforded of examining the cryſtalline lens of the eye, which is now known to be generally more denſe in the centre than at its ſurface.

Mr. Haukſbee, who was not aware of this difference, has eſtimated the refractive power of the cryſtalline lens, by forming it into a wedge by plates of glaſs, ſomewhat higher than I find it to be; but, with his accuſtomed accuracy, he remarked the apparent enlargement of an object, occaſioned by the variations of its denſity, which he was unable to explain.

In the table that follows, I have ſet down, not only the limits of refractive power in a cryſtalline lens of an ox, aſcertained by trial, but alſo an average, computed from the refractive denſity of a dried cryſtalline of an ox, of which the weight had been firſt taken in the recent ſtate, and the quantity of water loſt by drying alſo meaſured.

Table of refractive powers.

The table exhibits a ſeries of ſubſtances, arranged according to their refractive powers. That of the diamond is copied from Sir Iſaac Newton; of other bodies to which (on account of their being more denſe than glaſs) the machine for meaſurement would not apply, the refractive powers have been found by other means, for the ſake of furniſhing a more continued ſeries of ſubjects for comparative experiments. The reſt have been compared by this method; and their power, when expreſſed in numbers, actually meaſured.

TABLE I.

Diamond - -	2,44
Plumbago - -	—
Native ſulphur (double)	2,04
Glaſs, conſiſting of lead 6 and ſand 1 -	1,987
Glaſs of antimony -	1,98
Jargon - -	1,95
Spinelle ruby -	1,812
Arſenic - -	1,811
Muriate of antimony, variable	
White ſapphire -	1,768
Gum dragon - -	—
Iceland ſpar, ſtrongeſt	1,657
Sulphate of barytes (double) - -	1,646
Balſam of Tolu -	1,60
Guaiacum - -	1,596
Benzoin - -	—
Flint glaſs - -	1,586
Ditto - - -	1,583
Horn	—
Phoſphorus - -	1,579
Mica	—
Opium	—
Amber - -	1,547
Rock cryſtal (double)	1,547
Old plate glaſs -	1,545
Colophony - -	1,543
Box-wood	—
Bees-wax - -	1,542
Oil of ſaſſafras -	1,536
Red ſealing-wax	—
Spermaceti, cold	—
Sugar, after fuſion	—
Arſeniate of potaſh	—
Maſtic	—
Elemi	—
White wax (cold)	—
Oil of cloves -	1,535
Copal - - -	1,535
Anime - -	1,535
Radcliffe crown glaſs	1,533
Pitch	—
Centre of cryſtalline of fiſh, and dry cryſtalline of an ox -	1,530
Canada balſam -	1,528
Crown glaſs, common	1,525
Selenite - -	1,525
Caoutchouc -	1,524
Gum lac	—
Dutch plate glaſs -	1,517
Human cuticle -	—
Gum arabic -	1,514
Balſam of capivi -	1,507
Oil of amber -	1,505
Engliſh plate glaſs -	1,504
French plate glaſs -	1,500
Oil of nutmeg -	1,497
Sulphate of potaſh	1,495

Tallow

Tallow, cold -	1,49
Iceland ſpar, weakeſt	1,488
Camphor - -	1,487
Linſeed oil - -	1,485
Butter, cold -	1,480
Eſſence of lemon -	1,476
Oil of turpentine, common - -	1,476
———— rectified	1,470
Oil of almonds	—
—— olives -	1,469
—— peppermint -	1,468
—— lavender -	1,467
Tallow, *melted* -	1,460
Alum - - -	1,457
Spermaceti, *melted* -	1,446
Cryſtalline lens of an ox	1,447
to -	1,380
Computed average of ditto - -	1,430
Sulphuric acid -	1,435
Fluor ſpar - -	1,433
Nitric acid (ſp. gr. 1,48)	1,410
Alcohol - -	1,37
White of an egg -	1,36
Æther - -	1,358
Vitreous humour of an eye - -	1,336
Water - -	1,336
Atmoſpheric air (Haukſbee) -	1,00032

ON THE DISPERSION OF LIGHT.

This method is likewiſe applicable to ſhew the diſperſion of light.

The method above deſcribed for inveſtigating refractive powers, may alſo be employed with ſimilar advantage for inquiries into the diſperſion of light by different bodies, and the conſequences that reſult from their combined action.

The limit of perfect reflection is marked by a fringe of colours;

When a glaſs priſm is placed in contact with water, and brought near the eye, in ſuch a poſition that it reflects the light from a window, the extent of perfect reflection is ſeen to be bounded by a fringe of the priſmatic colours, in the order of their refrangibility.* The violent rays, being in this caſe the moſt refrangible, appear ſtrongeſt and loweſt, on account of the leſs obliquity that is requiſite for their reflection.

except when (the refractions being unequal) the diſperſions are equal.

But it may happen that two media, which refract unequally at the ſame incidence, may diſperſe equally at that incidence. Under theſe circumſtances, a pencil of rays paſſing from one of ſuch media into the other, will be refracted, without diſperſion of its colours. The boundary of priſmatic reflection would then be found a well defined line, free from colour, if the ſurface at which the reflected light emerges from the priſm, were at right angles to its courſe.

If the diſperſion by the rarer medium be greateſt the order of colours will be inverted.

When the diſparity of the diſperſive powers of the media is ſtill greater, it may alſo happen, that the uſual order of priſmatic colours will be reverſed; and then the red will appear ſtrongeſt and loweſt in the fringe, unleſs the colours ſo produced are counteracted by refraction at their emergence from the priſm.

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* Newton's Optics. Book i. part 2. Exp. 16.

Inſtance: F. glaſs and ol. ſaſſafras.

An inſtance in which the colours are ſo reverſed, may be ſeen by application of oil of ſaſſafras to a priſm of flint glaſs.

So high is the diſperſive power of this oil, that, in refractions from flint glaſs into it, the red rays are refracted more than the violet.

It muſt be obſerved that, in this experiment, when the angle of reflection within a triangular priſm exceeds 60°, the angle of emergence is ſuch as would alone occaſion the red rays to appear lowermoſt; but, when the glaſs uſed is rectangular, the refraction at emergence has an oppoſite effect; any reverſion of colour will therefore be in ſome degree corrected, and may not be ſeen, unleſs the diſperſive power of the medium in contact much exceeds that of the glaſs.

Crown glaſs and oil of turpentine.

A caſe of refraction with an inverted order of colours, has been obſerved by Dr. Blair,[*] in a compound object-glaſs where crown-glaſs was in contact with oil of turpentine. From trials with lenſes, he likewiſe inferred, that ſeveral other fluids have the ſame effect, when applied to that glaſs.

other oils, &c.

With this glaſs, and alſo with plate-glaſs, I have tried oil of turpentine, and many other fluids that afford a ſimilar reverſion of colours, as linſeed-oil, olive-oil, the eſſential oils of bergamot, lemon, lavender, pennyroyal, and peppermint, ſtrong nitric acid, and many artificial compounds that I ſhall preſently have occaſion to mention.

Fluor ſpar diſperſes very little

The diſperſive power of fluor ſpar is the leaſt of any ſubſtance yet examined; ſo that, although its refractive power is alſo remarkably low, (conſidering its great ſpecific gravity,) a priſm of fluor, in contact with water or alcohol, ſhows the priſmatic colours to be refracted in an inverted order.

alſo ponderous ſpar

With heavy ſpar, the inſtances of reverſion are very numerous, as its diſperſive power is low, and is accompanied with great refractive denſity. In the refractions from this ſpar into flint glaſs, and into all oils or reſins, I believe, without exception, the colours are ſeen reverſed.

and rock cryſtal

Rock cryſtal likewiſe diſperſes ſo little, that it exhibits the colours reverſed, when it is in contact with many ſubſtances of leſs refractive power than itſelf. I have tried it with Dutch plate-glaſs, with Canada balſam and balſam of capivi, with many oils eſſential and expreſſed, and have found the colours in all theſe caſes reverſed.

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[*] Edinb. Tranſ. Vol. III.

Metallic ſalts diſperſe much.

By ſolutions of metallic ſalts, a great variety of ſuch appearances may be produced. Moſt of theſe compounds have a highly diſperſive power; and many of them may be rendered ſufficiently denſe to occaſion reverſion, even when applied to flint-glaſs. In a more dilute ſtate, they may be uſed with crown-glaſs, or plate-glaſs, to produce the ſame effect. And ſince, when further diluted by a leſs diſperſive medium, they will alſo preſent an appearance of colourleſs refraction, we may, by examining the degree of dilution neceſſary for that purpoſe, compare the diſperſive powers of any ingredients contained in them, and may gradually extend our knowledge of this property to the elements of any bodies, however compounded.

Experiments of diluting them till no colour appeared.

As a ſpecimen of the method, I have in this way compared a few ſolutions of metals, and of other ſubſtances, that were each diluted till the limit of reflection appeared void of colour, when they were in contact with a rectangular piece of plate-glaſs; and, in the table which follows, I have expreſſed their refractive powers in that ſtate of dilution, as nearly as the eye can diſcern the diſappearance of colour.

TABLE II.

Table of their denſities in that ſtate.

	In Water.	In Alcohol.
Nitro-muriate of gold - - - -	1,364	1,390
Nitro-muriate of platina - -	1,370	
Nitrate of iron - - - - -	1,375	
Sulphuret of potaſh - - - -	1,375	
Red muriate of iron - - - -	1,385	
Nitrate of magneſia		
Nitric acid - - - - - -	1,395	
Nitrate of jargon		
Balſam of Tolu - - - - -	—	1,400
Acite of litharge (extract of lead) -	1,400	
Nitrate of ſilver		
Nitrate of copper		
Oil of ſaſſafras - - - - -	—	1,405
Muriate of antimony - - -	—	1,410
Nitrate of lime - - - - -	1,410	1,422
Nitrate of zinc		
Green muriate of iron - - -	1,415	
Muriate of magneſia - - - -	1,416	
——— of lime - - - - -	1,425	1,440
——— of zinc - - - - -	1,425	
Eſſence of lemon - - - - -	—	1,430
Balſam of capivi - - - - -	—	1,440

Gold and platina moſt increaſe the diſperſion; zinc leaſt.

It may here be ſeen, that ſeveral of the metals increaſe the diſperſive powers of nitric and muriatic acids, and conſequently exceed them in that reſpect. Of all theſe ſubſtances that I have yet tried, gold and platina are the moſt diſperſive. The leaſt diſperſive of the metals is zinc.

The earths differ much.

The earths alſo are found to poſſeſs this property in very different degrees: that of the jargon and magneſia differ but little from nitric acid in diſperſive power; but ſiliceous earth, on the contrary, is inferior to water.

Nitric acid diſperſes more than muriatic.

By comparing the ſalts formed with the nitric and muriatic acids, it appeared probable that the former had the higher diſperſive power; but a more direct compariſon could not be made by means of the rectangular piece of plate-glaſs, as muriatic acid could not be rendered ſufficiently denſe for ſuch a trial; I therefore made uſe of a triangular priſm of crown-glaſs, which is in itſelf leſs diſperſive than any plate-glaſs, and, from the relative poſition of its ſurfaces, occaſioned leſs correction of the colours. With this priſm, I found that ſtrong muriatic acid (having a refractive power 1,394) exhibited the colours reverſed; and that, when it was diluted till the limit of reflection appeared void of colour, its refractive power was reduced to 1,382. But the diſperſive power of nitric acid, when tried by the ſame priſm, proved to be greater; for this acid required to be diluted till its refractive power did not exceed 1,375, before the colour was wholly deſtroyed.

More oxygen ſeems to produce more diſperſion in the muriate of iron;

In the table it may be obſerved, that the red and green muriates of iron, though conſiſting of the ſame metal and acid, differ very much in diſperſive power; and, conſequently, that ſome caution will be neceſſary, in attempting to compare the different metals with each other by means of the ſalts containing them, as any difference obſerved may be owing in part to a difference in the quantity of acid to which they are united, and in part to their different proportion of oxygen.

but leſs in the oxidation of ſulphur.

A ſtriking inſtance of the latter is manifeſt, from a compariſon of ſulphur with the ſulphuric acid; for, while the former appears to exceed the metallic oxides in diſperſive power, the latter is inferior even to water.

The order of various bodies as to their diſperſive powers;

As I have likewiſe, at various times, made many experiments on diſperſion by means of wedges, in a manner nearly ſimilar to that employed by Mr. Dollond, Dr. Blair, and others, I have endeavoured to reduce the ſeveral ſubſtances thus examined to one table; but, as the limits of colour are in few inſtances ſufficiently well defined for accurate menſuration, I have not attempted to add any numerical eſtimate of their powers, but have merely aſcertained the order in which thay ſucceed each other; and, in the following table, have arranged them according to the exceſs of their effect on violet above red light, at a given angle of deviation.

TABLE III.

tabulated with their refractive powers annexed.

Order of diſperſive Powers.	Refr. Power.
Sulphur - -	2,04
Glaſs of lead ($\frac{1}{7}$ ſand)	1,987
Balſam of Tolu -	1,60
Oil of ſaſſafras -	1,536
Muriate of antimony	
Guaiacum - -	1,596
Oil of cloves -	1,535
Flint-glaſs - -	1,586
Colophony - -	1,543
Canada balſam -	1,528
Oil of amber -	1,505
Jargon - -	1,95
Oil of turpentine -	1,47
Copal - -	1,535
Balſam of capivi -	1,507
Anime - -	1,535
Iceland ſpar - -	1,657
Amber - -	1,547
Diamond - -	2,44
Alum - -	1,457
Plate-glaſs, Dutch	1,517
Ditto, Engliſh -	1,504
Crown glaſs -	1,533
Ruby (ſpinelle) -	1,812
Water - -	1,336
Sulphuric acid -	1,435
Alcohol - -	1,37
Sulphate of barytes	1,646
Selenite - -	1,525
Rock cryſtal - -	1,547
Sulphate of potaſh -	1,495
White ſapphire -	1,768
Fluor ſpar - -	1,433

Numerous achromatic combinations.

By compariſon of this table with the order of refractive powers, as contained in the firſt table, it will be ſeen how little correſpondence there is between them; and, accordingly, how numerous are the combinations by means of which a pencil of rays that paſſes through two media, may be made to deviate without diſperſion of its colours.

White light does not appear to be ſeparable by the priſm into *ſeven* nor into *three* colours.

I cannot conclude theſe obſervations on diſperſion, without remarking that the colours into which a beam of white light is ſeparable by refraction, appear to me to be neither 7, as they uſually are ſeen in the rainbow, nor reducible by any means (that I can find) to 3, as ſome perſons have conceived; but that, by employing a very narrow pencil of light, 4 primary diviſions of the priſmatic ſpectrum may be ſeen, with a degree of diſtinctneſs that, I believe, has not been deſcribed nor obſerved before.

Experiments by which the colours are ſhewn to be four.

If a beam of day-light be admitted into a dark room by a crevice $\frac{1}{10}$ of an inch broad, and received by the eye at the diſtance of 10 or 12 feet, through a priſm of flint-glaſs, *free from veins*, held near the eye, the beam is ſeen to be ſeparated into the four following colours only, red, yellowiſh green, blue, and violet; in the proportions repreſented in Fig. 3.

Remarkable facts relating to the ſpectrum.

The line A that bounds the red ſide of the ſpectrum is ſomewhat confuſed, which ſeems in part owing to want of power in the eye to converge red light. The line B, between red and green, in a certain poſition of the priſm, is perfectly diſtinct; ſo alſo are D and E, the two limits of violet. But C, the limit of green and blue, is not ſo clearly marked as the reſt; and there are alſo, on each ſide of this limit, other diſtinct dark lines, *f* and *g*, either of which, in an imperfect experiment, might be miſtaken for the boundary of theſe colours.

Spaces occupied by the colours;

The poſition of the priſm in which the colours are moſt clearly divided, is when the incident light makes about equal angles with two of its ſides. I then found that the ſpaces AB, BC, CD, DE, occupied by them, were nearly as the numbers 16, 23, 36, 25.

nearly the ſame in various bodies;

Since the proportions of theſe colours to each other have been ſuppoſed by Dr. Blair to vary according to the medium by which they are produced, I have compared with this appearance, the coloured images cauſed by priſmatic veſſels containing ſubſtances ſuppoſed by him to differ moſt in this reſpect, ſuch as ſtrong but colourleſs nitric acid, rectified oil of turpentine, very pale oil of ſaſſafras, and Canada balſam, alſo nearly colourleſs. With each of theſe, I have found the ſame arrangement of theſe four colours, and, in ſimilar poſitions of the priſms, as nearly as I could judge, the ſame proportions of them.

but is changed with the inclination.

But, when the inclination of any priſm is altered ſo as to increaſe the diſperſion of the colours, the proportions of them to each other are then alſo changed, ſo that the ſpaces AC and CE, inſtead of being as before 39 and 61, may be found altered as far as 42 and 58.*

Inviſible heat making rays on the confine of red;

* Although what I have above deſcribed compriſes the whole of the priſmatic ſpectrum that can be rendered viſible, there alſo paſs on each ſide of it other rays, whereof the eye is not ſenſible. From Dr. Herſchel's experiments (Phil. Tranſ. for 1800) we learn, that

Curious effects of the refraction of candle light;

By candle-light, a different ſet of appearances may be diſtinguiſhed. When a very narrow line of the blue light at the lower part of the flame is examined alone, in the ſame manner, through a priſm, the ſpectrum, inſtead of appearing a ſeries of lights of different hues contiguous, may be ſeen divided into five images, at a diſtance from each other. The 1ſt is broad red, terminated by a bright line of yellow; the 2d and 3d are both green; the 4th and 5th are blue, the laſt of which appears to correſpond with the diviſion of blue and violet in the ſolar ſpectrum, or the line D of Fig. 3.

and the light of electricity.

When the object viewed is a blue line of electric light, I have found the ſpectrum to be alſo ſeparated into ſeveral images; but the phenomena are ſomewhat different from the preceding. It is, however, needleſs to deſcribe minutely, appearances which vary according to the brilliancy of the light, and which I cannot undertake to explain.

on one ſide there are inviſible rays occaſioning heat, that are leſs refrangible than red light; and on the other I have myſelf obſerved, (and the ſame remark has been made by Mr. Ritter,) that there are likewiſe inviſible rays of another kind, that are more refracted than the violet. It is by their chemical effects alone that the exiſtence of theſe can be diſcovered; and, by far the moſt delicate teſt of their preſence is the white muriate of ſilver.

and diſoxygenating rays on the confine of violet.

To Scheele, among many valuable diſcoveries, we are indebted for having firſt duly diſtinguiſhed between radiant heat and light; (*Traité de l'Air et du Feu*, § 56, 57;) and to him alſo we owe the obſervation, that when muriate of ſilver is expoſed to the common priſmatic ſprectrum, it is blackened more in the violet than in any other kind of light. (§ 66.) In repeating this experiment, I found that the blackneſs extended not only through the ſpace occupied by the violet, but to an equal degree, and to about an equal diſtance, beyond the viſible ſpectrum; and that, by narrowing the pencil of light received on the priſm, the diſcoloration may be made to fall almoſt entirely beyond the violet.

It would appear therefore, that this and other effects uſually attributed to light, are not in fact owing to any of the rays uſually perceived, but to inviſible rays that accompany them; and that, if we include two kinds that are inviſible, we may diſtinguiſh, upon the whole, ſix ſpecies of rays into which a ſun-beam is diviſible by refraction.

VI.

An Account of Dr. YOUNG'S *Harmonic Sliders. From his Paper in the Journals of the Royal Inſtitution, p.* 261.

Utility of the doctrine of combined undulations in explaining cotemporary ſounds; and the tides.

THE combination of undulations, however cautiouſly the world may adopt its application to the explanation of optical phenomena, is of acknowledged utility in illuſtrating the phenomena of muſical conſonances and diſſonances, and of undeniable importance in accounting for many of the phenomena of the tides. Each tide is an undulation on a large ſcale; and, ſuppoſing the general form of the ocean, in conſequence of the attraction of a diſtant body, to coincide with that of an oblong ſpheroid, as it is found by calculation to do, the ſection of the ſurface of each tide, if conceived to be unbent from the circular form and extended on a plane, would form the harmonic curve. (Young's Syllabus, IV. 151. 155.) It is remarkable that the motions of the particles of the air in ſound have been generally ſuppoſed in theory to correſpond with the ordinates of this ſame curve, and that there is alſo experimental reaſon to believe, that the pureſt and moſt homogeneous ſounds do in fact agree very nearly with the law of this curve. It is therefore by far the moſt natural as well as the moſt convenient to be aſſumed, as repreſenting the ſtate of an undulation in general; and the name of theſe harmonic ſliders is very properly deduced from the harmonic curve.

The harmonic ſliders viſibly exhibit the proceſs of nature in the junction of undulations.

By means of this inſtrument, the proceſs of nature, in the combinations of motion which take place in various caſes of the junction of undulations, is rendered viſible and intelligible, with great eaſe, in the moſt complicated caſes. It is unneceſſary to explain here, how accurately both the ſituations and motions of the particles of air, in ſound, may be repreſented by the ordinates of the curve at different points: it is ſufficient to conſider them as merely indicating the height of the water conſtituting a tide, or a wave of any kind, which exiſts at once in its whole extent, and of which each point paſſes alſo in ſucceſſion through any given place of obſervation. We have then to examine what will be the effect of two tides, produced by different cauſes, when united. In order to repreſent this effect, we muſt add to the elevations or depreſſions in conſequence of the firſt tide, the elevations or depreſſions

Explanation applied to tides.

preſſions in conſequence of the ſecond, and ſubtract them when they counteract the effect of the firſt: or we may add the whole height of the ſecond above any given point or line, and then ſubtract, from all the ſums, the diſtance of the point aſſumed below the medium.

Exhibition of a ſimple tide,

To do this mechanically is the object of the harmonic ſliders. The ſurface of the firſt tide is repreſented by the curvilinear termination of a ſingle board, Plate VI. Fig. 1. The ſecond tide is alſo repreſented by the termination of another ſurface; but, in order that the height at each point may be added to the height of the firſt tide, the ſurface is cut tranſverſely into a number of ſeparate pieces or ſliders, which are confined within a groove or frame, and tightened by a ſcrew, Fig. 2. Their lower ends are ſituated orginally in a right line; but, by looſening the ſcrew and moving the ſliders, they may be made to aſſume any other form: thus they may be applied to the ſurface repreſenting the firſt tide; and if the ſimilar parts of each correſpond, Fig. 3, the combination will repreſent a tide of twice the magnitude of the ſimple tides.

and of combinations which either increaſe, or modify, or deſtroy each other.

The more the correſponding parts are ſeparated, the weaker will be the joint effect, Fig. 4; and, when they are furtheſt removed, the whole tides, if equal, will be annihilated, Fig. 5. Thus, when the general tide of the ocean arrives by two different channels at the ſame port, at ſuch intervals of time that the high water of one would happen at the ſame inſtant with the low water of the other, the whole effect is deſtroyed, except ſo far as the partial tides differ in magnitude. The principle being once underſtood, it may eaſily be applied to a multiplicity of caſes: for inſtance, where the undulations differ in their dimenſions with regard to extent. Thus, the ſeries of ſliders being extended to three or four alternations, the effect of combining undulations in the ratio of 2 to 1 of 3 to 1, of 2 to 3, of 3 to 4, may be aſcertained, by making a fixed ſurface, terminating in a ſeries of curves, that bear to thoſe of the ſliding ſurface the ratio required: and, by making them differ but ſlightly, the phenomenon of the beating of an imperfect uniſon in muſic may be imitated, where the joint undulation becomes alternately redoubled and evaneſcent. In Fig. 6, the proportion is that of 17 to 18, and the curvilinear outline repreſents the progreſs of the joint ſound from the greateſt degree of intenſity to the leaſt, and a little beyond it.

The principle is applicable to a variety of caſes,

undulations differing in extent.

The beat of an imperfect uniſon.

VII. *Obſervations*

VII.

Obſervations on the Appearances produced by the Colliſion of Steel with Hard Bodies. By Mr. DAVY, *Lecturer on Chemiſtry at the Royal Inſtitution*[*].

Hawkſbee's exp. of flint and ſtee in vacuo.

I. MR. HAWKSBEE long ago ſhewed, that no ſparks could be produced by the colliſion of flint and ſteel[†] in the exhauſted receiver of an air pump, and that in this caſe a faint light only was perceived. And, ſince his time, the ſame obſervation has been very often made.

Sparks thus produced in air, are owing to combuſtion of the ſteel.

The developement of the theory of combuſtion has clearly ſhown that the vivid ſparks obtained from ſteel in the atmoſphere, are owing to the combination of the ſmall abraded and heated metallic particles with oxigen. But it has been a matter of doubt whether, in the experiment made in vacuo, the faint luminous appearance is owing wholly to the light produced by the fracture and abraſion of the parts of the flint, or only partly to this cauſe, and partly to the ignition of the minute filaments ſeparated from the ſteel.

Light in vacuo is moſt vivid with a thin ſharp flint.

II. I have often found, that when a fine and thin flint, which may be eaſily broken, is uſed for the colliſion in vacuo, the light is much more vivid than when a thick and ſtrong one is employed: and with a ſtrong flint, but juſt ſharp enough to give ſparks with ſteel in the atmoſphere, it is ſeldom that any light at all is produced in the exhauſted receiver. Theſe facts ſeem to ſhew that the abraded particles of ſteel are not rendered at all luminous by colliſion, except in conſequence of combuſtion; and the opinion is almoſt fully proved by the following experiment, which was made in the courſe of a lecture on the properties of light, in the theatre of the Royal Inſtitution, and which has been ſince often repeated.

The colliſion does not make the ſteel red hot.

Pyrites inſtead of flint, gave n light in vacuo.

III. A thin piece of iron pyrites[‡] (ſulphuret of iron) was inſerted in a gunlock in the place of the flint. It gave by colliſion in the atmoſphere very vivid ſparks; which were chiefly white, from the combuſtion of the particles of the ſteel; but ſometimes mixed with a few red ſparks from the combuſtion of the particles of the pyrites. The gunlock was introduced under the receiver of an air pump, and the exhauſtion was made till the mercury in the ſhort gage ſtood at about $\frac{6}{10}$ of an inch. The lock was then ſnapped, but no light whatever was perceived; and the phenomenon was uniform, every precaution being taken to render the room dark, and to preſerve the apparatus in order.

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[*] Journal of the R. I. 264.

[†] Philoſophical Tranſactions, Vol. XXIV. p. 2165.

[‡] The etymology of the name of this ſubſtance ſhews that its property of giving fire by colliſion was very anciently known. It was uſed in the old gunlocks, with the revolving wheel, for inflaming the priming.

Queſtion. Can ſteel burn in air if not firſt white hot?

IV. It is well known that in common caſes the fineſt ſteel wire does not burn with a white light or ſparks in the atmoſphere, unleſs it have been previouſly heated to a degree much above that of the red heat; it conſequently at firſt view appears extraordinary, that the particles ſeparated from the gunlock ſhould be heated ſo as to burn vividly in air, and yet not ſo as to appear ignited in vacuo; for it is not eaſy to conceive that they emit light, which from the minuteneſs of their volume * cannot be perceived; or to ſuppoſe that the opacity of the metallic ſubſtances ſhould hinder light generated at their points of contact from being viſible. I had formerly ſuppoſed, in reaſoning upon the phenomenon of the colliſion of flint and ſteel, that † heat and light might in common inſtances be only accidentally coexiſtent; and that in certain caſes very high temperatures might be produced without cauſing the appearance of light. At preſent however I am inclined to believe, that the phenomena may be adequately accounted for upon principles that coincide with the common facts relating to the production and communication of heat.

If it cannot, why is not this white heat ſeen in vacuo?

Explanation. Oxidation at temperatures far below ignition, may develope more heat than can be conducted off, and may produce vivid combuſtion;

Mr. Stodart ‡ has ſhewn, that when ſteel is gradually heated it begins to change colour at about 430° Fahrenheit. And this change of colour is occaſioned by its combination with oxigen, and, as there is every reaſon to believe, muſt be connected with the evolution of heat. At about 600°, a temperature much below that of ignition, it oxidates rapidly, and becomes covered with a bluiſh grey coating. And though in theſe caſes of oxidation the heat evolved at the ſurface of

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* Or the ſhort time of its emiſſion.—N.

† Nicholſon's Journal, 4to, Vol. III. p. 517.

‡ Nicholſon's Journal, 4to, Vol. IV. p. 130.

the metal is not ſufficient to raiſe the temperature of ſteel wire, or a ſteel plate, ſo as to cauſe it to enter into the vivid combuſtion, yet in acting upon ſuch a minute filament as that ſtruck off in the gunlock, it may be ſufficient to keep up the proceſs of oxidation till it becomes ſo vivid as to occaſion the ſtrongeſt heat and light. Beſides, the ſurface of this filament is very great as compared with its bulk, and the oxide produced upon it is leſs likely to form a coat which might defend the interior parts from the action of the air *.

in caſe the ſurface be very great in proportion to the maſs.

It would not be difficult to find many analogous inſtances, in which the progreſs of oxidation is dependent upon the maſs of the combuſtible body, or rather upon the relation of this maſs to ſurface; thus, a very thin and ſmall bit of phoſphorus will inflame ſpontaneouſly, and burn with the vivid light when wrapped up in filaments of fine cotton; whilſt a thicker and larger piece will only ſhine with the feeble blue light: and though a large maſs of zinc may be melted in the atmoſphere without inflaming, a ſmall and thin ſhaving will burn vividly long before it is heated to the temperature of fuſion.

Other inſtances. Phoſphorus and zinc readily take fire in ſmall maſſes.

V. In conſidering the general phenomena of the production of heat and light, by mechanical means, it is difficult to conceive that any conſiderable increaſe of temperature can be produced on a metallic ſurface by a ſingle colliſion; for the conducting power of the metals is ſuch as would ſpeedily cauſe the heat to be communicated to the contiguous parts; and even in the caſe of the abraſion of minute particles, though the time required for their ſeparation from the maſs is to us imperceptible, yet it muſt be ſufficient to enable them to give out to it a portion of their heat.

Not probable that much heat ſhould be produced by a ſingle colliſion.

The bodies that become luminous by being ſtruck or rubbed together in vacuo, or in gaſes that do not contain oxigen, or under water, ſuch as fluate and carbonate of lime, ſiliceous ſtones, glaſs, ſugar, and many of the compound ſalts, are both electrics *per ſe*, and phoſphoreſcent ſubſtances; ſo that the flaſhes they produce are moſt likely occaſioned, partly by

Luminous appearances, from electricity or phoſphoreſcence, by friction.

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* In turning very fine work of ſteel in the lathe, ſo as to afford ſhavings or threads much thinner than one-thouſandth of an inch, I found that this metallic wool very readily caught fire at a candle, and burned throughout in quantities of a cubic inch or more: But it was ſcarcely ſo much oxided as to have loſt its flexibility after this combuſtion.—N.

the electricity excited on their ſurfaces by the friction, and partly by their phoſphoreſcence, which is generally occaſioned by moderate degrees of heat. It is not however improbable that in ſome caſes, by the colliſion of very hard ſtony bodies, which are bad conductors of heat, there may be an actual ignition of abraded particles; and the ſuppoſition is countenanced by various facts. Mr. T. Wedgwood found that a piece of window glaſs, when brought in contact with a revolving wheel of grit, became red hot at its point of friction, and gave off luminous particles which were capable of inflaming gunpowder and hidrogen gas [*]. And we are informed by a late voyager [†], that the natives of Oonalaſhka light their fires by ſtriking together two pieces of quartz, their ſurfaces being previouſly rubbed with native ſulphur, over dry graſs.

Caſes of actual ignition:

VIII.

Deſcription of a Blow-pipe by Alcohol, having a ſafety Valve, with other Advantages. Conſtructed by Mr. BENJAMIN HOOKE, *Fleet Street.*

To Mr. NICHOLSON.

SIR,

I HAVE taken the liberty of troubling you with a drawing and deſcription of the *blow-pipe by alcohol* as I make it, which perhaps poſſeſſes the following advantages over that deſcribed in your number of September laſt; *viz.* Being furniſhed with a ſafety valve, to prevent accidents; having only one lamp (the wick of which being pretty large, anſwers both for heating the alcohol and for affording a ſtrong blaſt when drawn through it); and I think ſuperiority as to form and appearance.

Blow-pipe with a ſafety valve and only one flame.

Should you eſteem it worthy a place in your valuable Journal, the inſertion will oblige,

SIR,

Your very humble ſervant,

BENJAMIN HOOKE.

159, *Fleet Street,*
Nov. 20, 1802.

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[*] Phil. Tranſ. 1792, p. 45.

[†] Sauer's Account of Billings's Expedition to the northern parts of Ruſſia, p. 159.

A is a hollow ſphere * for containing alcohol, reſting upon a ſhoulder in the ring O, Plate V. Deſcription.

B is a bent tube with a jet at the end, to convey the alcohol in the ſtate of vapour into the flame at Q, this tube is continued in the inſide up to C, which admits of A being filled nearly, without any alcohol running over.

D is a ſafety valve, the preſſure of which is determined at pleaſure, by ſcrewing higher or lower on the pillar E, the two milled nuts F and G carrying the ſteel arm H, which reſts on the valve.

I is an opening for putting in the alcohol.

K is the lamp, which adjuſts to different diſtances from A, by ſliding up or down the two pillars L. The diſtance of the flame Q from the jet, is regulated by the pipe which holds the wick being a little removed from the centre of the braſs piece M, and of courſe revolving in a circle.

N the mahogany ſtand.

IX.

Deſcription of a Joint applied to Tubes uſed for conveying Steam under conſiderable Preſſure. W. N.

THE valuable communication of my correſpondent N. N. in the laſt number, has fixed my recollection upon ſeveral parts of my hydraulic and pneumatic apparatus, in which, though they may be called bagatelles as far as relates to the magnitude of the inventive powers required to make them, yet their utility may be ſuch as in many inſtances to prevent leſs eligible contrivances from being adopted. Recollection of ſome apparatus.

In the firſt place, I find among ſome braſs work the ingenious joint, Plate II. Fig. 2, lately contrived by himſelf, and before him by Mr. Webſter, but in this caſe certainly made many years ago by ſome unknown mechanic. My piece, which is for ſpouting fluids, is prevented from being driven aſunder by a wire that paſſes through its axis, and has a ſmall nut and waſher faced with leather. Early invention of the joint for pneumatic apparatus.

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* If the bottom is made flat inſtead of ſpherical, the action of the flame will then be greater.

Another joint for ſtrong ſteam.

The apparatus of which I have given a drawing and ſection in Plate VII. was made for conveying ſteam from a boiler to the ſteam engine, and for many other philoſophical purpoſes, in which this agent is very uſeful, though hitherto not much applied to objects of this nature. As I have found it very commodious, I ſhould not be diſpoſed to make any remarks on the contrivance itſelf, if its reſemblance to ſo many things that I have ſeen, both in organization and effect, did not make it proper to ſay, that I ſhould have let it paſs unpubliſhed (among the many arrangements, whether old or new, which every practical mechanic finds himſelf induced to adopt in his operations) had not the conſideration of utility made me ſuppoſe it might prove acceptable.

Delineation. A wide conical cock with joints in the tubes, preſſed together with a ſcrew and a diſh ſpring.

In Fig. 1, 2, and 3, the ſame letters of the alphabet denote the ſame things; Fig 1 exhibiting a view, Fig. 2 a ſection, and Fig. 3 the plan of ſome of the parts. The letter *e* ſhews part of an upright tube conveying ſteam from the boiler into the body of the cock *c* by its hollow cone *m*, which is open beneath and cloſed above, and has a ſide aperture *n* communicating, when duly placed, with the pipe *h i*. The cone is preſſed into its place by the cap *b* ſcrewed on the body of the cock *c* at *r*. The cap does not preſs immediately upon the upper ſurface or ſhoulder of the cone, becauſe the motion would in that caſe be hard and uncertain; but it acts by the intervention of a ſpring *o* in the ſhape of a diſh or ſlightly concave place, which keeps it to its bearing with any required degree of preſſure accordingly as the cap is more or leſs ſcrewed down, and always with a moſt ſmooth and pleaſant action without the leaſt jerk or obſtacle. The cock is turned by the wooden handle or lever *a*, and is very ſecure on account of the large ſurface of the convex and concave cones which remains imperforated.

The pipe between *h* and *i* is connected by a joint, in which *g* is a looſe cap milled on the outſide, and having a concave ſcrew on its inner ſurface. It is ſcrewed upon the fixed piece *f*, which is turned to fit it: In this laſt piece is a cylindric concavity ſurrounding the orifice of the pipe *h* (ſee *f*, Fig. 3), into which the piece *p*, at the extremity of the pipe *i*, looſely fits. Upon the circular face of *i* are turned three or four grooves, and the like on the correſpondent face of *f*. Theſe are of uſe to ſecure a ſmall piece of tallowed linen cloth put between them when applied together. Laſtly, there is a diſh ſpring *o* between *p* and *g*. When *g* (which ſlides looſely upon the pipe *i*) is ſcrewed upon *f*, it preſſes *p* by the intervention of the ſtiff diſh ſpring againſt the interior of *f*, and makes a ſecure junction; but the pipe *i*, repreſented in Fig. 1 as pendant, or in a vertical poſition, may be moved round in the ſame plane to any elevation or depreſſion whatever.

The cap *d* being ſcrewed on the body of the cock at *t*, conſtitutes a joint of the ſame kind, by means of which the pipe *i* may be moved into any horizontal poſition: Conſequently, by theſe two motions, that pipe may be placed in every poſſible ſituation.

If we ſuppoſe the pipe *i* that proceeds immediately from the cock, to be of conſiderable length, as for convenience it is required to be, its poſſible changes of poſition will not be ſufficient to adapt it to the uſual operative purpoſes. It may therefore, in ſeveral caſes, be convenient to apply two other ſimilar joints *k* and *l* at right angles with each other, by means of which it will be immediately ſeen, that the exterior pipe *w* may be placed in any required direction, not merely with regard to a point immediately above the boiler, but with regard to another moveable point at an equal diſtance from the fire.

The diſh ſpring generally recommended.

I ſhall conclude this paper by obſerving, that the bended waſher or diſh ſpring is uſed in the vice and ſome other mechanical tools, but that it far from being as much uſed as it ought to be. For there is ſcarcely any action of pinching, preſſing, or binding, in which it would not be advantageous, and would, in many caſes, prevent ſerious miſchief and inconveniences.

Their experiments on it, which are in favour of the principle.

lating it ſenſibly, ſo that the difficulty of not producing ſome inequalities of curvature in large glaſſes muſt be very great. Perhaps it is impoſſible to avoid this imperfection, but it may be corrected: and our colleague affirms, that the effect of the imperfections of the four interior ſurfaces of the three glaſſes may be diminiſhed conſiderably, by the introduction of a diaphanous fluid between them. Experiments alone can decide the fact. We took therefore an achromatic teleſcope three feet long, the aperture of which was about three inches. The two lenſes compoſing the object glaſs, being about half an inch diſtant from each other, we introduced between them a thin Bohemian glaſs not wrought. In this ſtate it is obvious the teleſcope muſt be very bad; and accordingly, on directing it to ſome writing, we were obliged to bring this within eleven yards and half of it, to diſtinguiſh the letters. Having then poured pure water between the object glaſſes, ſo as to fill the intervals between them, we found the letters not more difficult to be diſtinguiſhed at the diſtance of ſixty-two yards. We might have purſued our experiments further, but this was ſufficient to eſtabliſh the principle, which was all we had in view."

To make a perfect object glaſs, the refractive power of the interpoſed fluid ſhould be the ſame with that of the glaſs.

Water reduces the confuſion to one ſeventh;

but oil, though its power is nearer that of glaſs, does not reduce it ſo much.

At this time I was ordered to Breſt, which put a ſtop to my purſuit. In my memoir I had advanced, that the confuſion ariſing from the irregularity of the glaſs of a teleſcope, was to the confuſion which takes place when the glaſs is immerſed in a fluid, as the refraction of the glaſs is to the difference of refraction between the glaſs and the fluid: conſequently, if the fluid have the ſame refractive power as the glaſs, no alteration in the diſtinctneſs of the object will be perceptible; but if the glaſs be immerſed in water, the confuſion will be about a ſeventh only of what would have taken place, had the object been ſeen directly through the irregular glaſs. This calculation, it is true, ſuppoſes the glaſs to be in perfect contact with the fluid; but there are obviouſly many cauſes by which this contact may be influenced. It may have been owing to ſome ſuch cauſe, that oil, the refractive power of which differs leſs from that of glaſs, was found by experiment not to anſwer ſo well as water; for writing, which was legible at the diſtance of 130 feet when water was employed, could be read only at eighty-eight feet when oil was interpoſed between the glaſſes *.

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* On this ſubject, ſee Philoſ. Journal, 4to, III. 000.

By the Regiſters of the Academy of Sciences, January the 6th, 1788, it appears, that Grateloup adopted the ſame method. He did not make uſe of a fluid interpoſed between the glaſſes, however, but of maſtic. This ſubſtance had long been in uſe among jewellers, for cementing together ſtones, which it did in ſuch a way, that it was ſcarcely poſſible to diſtinguiſh the two ſo united from a ſingle ſtone; and hence Grateloup conceived, it would render his object glaſſes as it were one ſolid piece, without being liable to evaporation like a fluid, while it corrected the errors of their interior ſurfaces. Glaſſes cemented together with maſtic, however, do not anſwer, at leaſt for ſea voyages, as change of temperature, and the ſea air, affect the maſtic very much. On this account I have preferred reſin, and even turpentine the moſt fluid and tranſparent I could get. Chemiſts perhaps may find ſubſtances ſtill preferable to theſe, and it is an object certainly worthy their attention. Dr. Blair tried a great number of ſolutions of metals and ſemimetals, and he ſays, that certain ſalts, particularly ſal ammoniac diſſolved in water, give it a conſiderable power of diſperſion. The oxigenated muriatic acid, too, poſſeſſes this quality in a great degree; but butter of antimony has a ſtill greater effect, for one priſm of this is equal to three of crown glaſs of ſimilar dimenſions. Dr. Blair conſtructed an object glaſs with crown glaſs and butter of antimony, but he obſerved, that it occaſioned irradiations, which led him to prefer oil of turpentine or other eſſential oils *.

Grateloup uſed maſtic between the glaſſes.

But change of temperature, and the ſea air, injure maſtic.

Reſin therefore preferable, or turpentine.

Dr. Blair tried various ſubſtances; ſome ſalts, and oxigenated muriatic acid, had a conſiderable power of diſperſion; and muriate of antimony had ſtill more; but this occaſioned irradiations, whence he preferred eſſential oils.

The uſe of achromatic glaſſes applied to graduate circles or ſegments of circles for meaſuring angles, and the defects to which inſtruments of this kind are liable, may be paſſed over as not to the preſent purpoſe, which is the beſt mode of meaſuring very ſmall angles.

Buffon, who had paid ſome attention to the formation of rock cryſtals, finding no indication of a double refraction in the experiments which I made on that of Madagaſcar, in 1770, thought this tranſparent quarter to be of a different nature from the other cryſtals; but before he made up his mind on the ſubject, he deſired me to examine it afreſh with a view to this property.

Buffon ſuſpected the double refraction of the rock cryſtal of Madagaſcar might have been overlooked.

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* An ample abridgement of Dr. Blair's paper is given, Phil. Journal, 4to, I. 1.

Beccaria publiſhed a curious work on the refraction of cryſtals.

Huygens miſtaken in calling the refraction irregular;

as it uniformly depends on the internal ſtructure of the cryſtal.

Beccaria cut rock cryſtal in different directions;

whence he obtained certain laws of their refraction.

It may not be improper here to give a brief abſtract of a very curious work, which Beccaria publiſhed in Italian about this time, as I learned from it the direction in which it was neceſſary to cut my priſms, to be certain of obtaining the effects I ſought. Huygens, ſays Beccaria, who treats very fully on the double refraction of Iceland cryſtal, adds, that rock cryſtal poſſeſſes the ſame property, but in a leſs ſenſible degree. I ſaw that the refraction which Huygens calls irregular, bears a conſtant relation in the Iceland cryſtal to its internal ſtructure; for the effect of this refraction is to divert the rays in the direction of the ſaliant angles, which are equal in the whole piece, and in each of its component parts. From inſpecting the figure of rock cryſtal, I inferred its internal ſtructure. In fact, I conſidered it as ſimilar to artificial cryſtallizations formed by the aggretation of little laminæ parallel to the faces of the cryſtals. Suſpecting Huygens of inaccuracy in aſſerting, that priſms of rock cryſtal had always a double refraction, whatever their ſections were, I cut a piece of rock cryſtal, of a very irregular figure, in the direction of its axis, and ſo as o divide two of its oppoſite faces into two equal parts. I cut another likewiſe in the direction of its axis, but ſo that the ſection paſſed through the ſummits of two oppoſite angles. A third afforded me triangular priſms, one of the faces of which was one of the faces of the cryſtal itſelf, while each of the other two faces terminated on one ſide at one of the contiguous angles, and on the other at the axis of the cryſtal. A fourth was divided into equilateral priſms, two of the faces of which were equally inclined to the axis, and the third was parallel to a plane continued through the axis. From the obſervations made with theſe priſms, I obtained a very ſimple law, of which Newton ſays nothing, and the contrary of which is advanced by Huygens. 1. That the double refraction does not ſubſiſt in all the different priſms that may be cut from rock cryſtal, 2. That the ray of light which traverſes rock cryſtal in a place perpendicular to the axis undergoes two refractions, is divided into two, and gives two images, if not completely yet ſenſibly diſtinct. 3. That this diſtinctneſs of the two images diminiſhes in proportion as the direction of the ray approaches to that of the axis of the cryſtal. 4. That the double refraction and diſperſion of the two images ceaſe entirely, when the direction of the ray is parallel to the axis, or nearly parallel to it. Hence it follows, that in cutting a lens of rock cryſtal for optical purpoſes, we muſt take pieces parallel to the baſe of the cryſtal, ſo that the axis of the lens coincides with that of the cryſtal, or is at leaſt parallel to it. Thus far Beccaria.

The refraction of the cryſtal is ſingle in the direction of its axis.

When we would apply to practice this properly in rock cryſtal of doubling the images of objects, the colours that ariſe from the priſmatic figure neceſſary to be given to it muſt be corrected by a glaſs priſm, or the images will not appear well defined. Theſe two priſms united together may be called a *double refracting achromatic medium*, but in this ſtate it can ſerve only to meaſure the ſmall angle given by the double refraction: yet it is eaſy to conceive, that on applying two double refracting mediums one on the other, we may vary the effect of the double refraction at pleaſure by a circular movement, as in the inſtrument for meaſuring colours which I laid before the Academy. Thus this double refraction, which has been deemed detrimental to the conſtruction of optical inſtruments, is in fact advantageous for the menſuration of ſmall angles, as I convinced the Academy, February the 25th, 1777, by an inſtrument conſtructed on this principle, which gave the meaſure of ſmall angles with a degree of preciſion, for which we could ſcarcely have hoped. This inſtrument, however, had the inconvenience of giving four images of one object, which occaſioned a conſiderable loſs of light; and the leſs light we have, the leſs the accuracy with which the point of contact can be obſerved.

A priſm of rock cryſtal muſt be corrected by one of glaſs:

the two forming a *double refracting achromatic medium.*

Inſtrument for meaſuring ſmall angles by means of two of theſe mediums.

Its defect.

Endeavouring to remedy this imperfection, I ſoon diſcovered a more ſimple conſtruction for the inſtrument, which I laid before the Academy the ſame year. In this only one double refracting achromatic medium is required, which is made to move along the inſide of any teleſcope in the axis of the object glaſs. The value of the double refraction is firſt to be determined by experiment, as in the former micrometer, the achromatic medium being placed againſt the object glaſs. It is then to be moved from the object glaſs toward the eye glaſs. The angle of the double refraction will now be what it was in the former ſituation, but the ſeparation of the images will be in the ratio that the diſtance of the refractive medium from the focus bears to the focal diſtance. Suppoſe for example the double refraction to be twenty-one minutes, and the focal diſtance of the object glaſs to be three metres: if the

This remedied by uſing one medium, made moveable along the axis of the teleſcope.

Mode of uſing this micrometer.

achromatic medium be brought nearer by two thirds of this diſtance, the double refraction will occaſion only a ſeparation of ſeven minutes between the images. Thus if we would meaſure the diameter of an object, we muſt move the achromatic medium toward the focus, till the two images of the object are ſeen preciſely in contact; when, having the angle of double refraction given, by previous experiment, the focal diſtance of the object glaſs, and the diſtance of the achromatic medium from the focus, the diameter will be given by the rule of proportion. It is obvious, that great care muſt be taken in determining the angle of double refraction at firſt with preciſion, as on this will depend the accuracy of all ſubſequent meaſures with the inſtrument.

The inſtrument farther improved by uſing two priſms of rock cryſtal cut in oppoſite directions.

The effect of the double refraction doubled,

by means of two equal priſms.

Having thus improved the conſtruction of this micrometer, I found that the glaſs priſms, intended to correct the diſperſion of the priſm of rock cryſtal, left a refraction more and more perceptible in proportion as the double refracting medium was carried farther from the eye glaſs. I then availed myſelf of Beccaria's diſcovery, and cut my two priſms ſo that the firſt was in the direction of the double refraction, and the ſecond in that in which the double refraction is not perceptible: by theſe means I had a double refracting medium abſolutely exempt from colours and refractions. I did not ſtop here, for I was deſirous of extending the effect of the double refractions ſo as to meaſure the diameter of the ſun; and I accompliſhed this, which appeared to exceed the known power of the double refraction of rock cryſtal, which does not go beyond twenty minutes, when it is cut in the moſt advantageous priſmatic ſhape. For this I employed two equal priſms, cut in the direction moſt ſuitable to my purpoſe; and on placing them in oppoſite directions I found, that the double refraction was not perceptible; but on reverſing their directions the double refraction of each priſm was nearly doubled, ſo that I obtained two images ſeparated by an interval of forty minutes.

This inſtrument difficult to execute.

Hence the author was led to avail himſelf of Euler's method.

Effect of the

I ought not to omit, that in this new conſtruction there are difficulties of execution not eaſy to ſurmount, which may have been one reaſon why theſe inſtruments, ſo uſeful to navigators and in certain very nice aſtronomical obſervations, have not been adopted. This induced me at length to adopt Euler's method. In the conſtruction of achromatic object glaſſes I found I could increaſe or diminiſh the abſolute effect of the double refraction within certain limits, by means of the interval between the glaſſes of differing refracting powers: the ſeparation of the images at the focus being ſo much the greater, as the interval is larger, when the flint glaſs is the firſt of the object glaſſes; and leſs, when it is the ſecond.

double refraction increaſed by increaſing the interval between the object glaſſes when the flint glaſs is firſt; the reverſe when it is the ſecond.

Conformably to theſe new principles I have had two teleſcopes with a double refracting medium conſtructed under my own inſpection, which General Gantheaume will employ for determining the poſition of his ſhips, and to find whether he be approaching any he may meet with at ſea *.

The uſes of an inſtrument for meaſuring very ſmall angles with preciſion are too well known for me to deſcribe its advantages. The officers of the Engliſh navy are ſo fully aware of them, that they have uſed for ſome years Ramſden's eye glaſs micrometer, though this anſwers the end but imperfectly, becauſe it does not give the meaſure of the angle, and becauſe of the bad effect of the parallax produced by the decuſſation of the rays that enter the eye. This defect is more ſenſible in Ramſden's eye glaſs micrometer than in Bouguer's heliometer. The officers who have compared my inſtrument with Ramſden's, of which there were ſeveral on board the Spaniſh ſhips with our Breſt fleet, agree that the celebrated Engliſh artiſt has very imperfectly accompliſhed the object he propoſed; and Bouguer's heliometer could unqueſtionably be preferable for naval uſe, becauſe it has a leſs ſenſible parallax, and gives the meaſure of ſmall angles, ſo important for determining the diſtances of ſhips from their known dimenſions.

Defects of Ramſden's eye glaſs micrometer.

Bouguer's heliometer preferable.

Table of the proportions which the magnitude of an object bears to its diſtance: calculated according to the rule, which is eaſily demonſtrated, that, in every right angled triangle, the tangent is to the radius as the magnitude of an object is to its diſtance from the centre of the eye, when, under an angle known, the diſtance forms a right angled triangle with the object.

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* The General has made an advantageous report of this inſtrument, in his account of the chace of the Swiftſure, which he captured. This inſtrument is ſo difficult to execute, that I know only one perſon, Citizen Narci, who is capable of giving rock cryſtal the priſmatic form in the proper direction for obtaining the double refractions neceſſary to the goodneſs of the micrometer. He has made ſeveral, which have given me perfect ſatisfaction.

only one perſon yet found capable of making this micrometer.

EXAMPLE.

General formula for calculating diſtances.

Suppoſe the angle be meaſured under which we perceive an object of the magnitude of one toiſe, placed vertically ſo as to repreſent the tangent, the radius of which is the diſtance from the eye; if this angle be of 30′ we ſhall have the following proportion: the tangent of 30′ is to the radius, as one toiſe is to the diſtance. By the tables of logarithms we ſhall have this diſtance by ſubtracting from the logarithm of the coſine the logarithm of the tangent of 30′, which is 7.940858: the logarithm reſulting from this ſubtraction is 2.059142; and the number anſwering to this logarithm is 114.6 nearly; conſequently the diſtance from the eye to the object, the magnitude of which is ſuppoſed to be one toiſe, is 114 toiſes ſix tenths; but in the uſe of the inſtrument fractions may be neglected.

It is proper to obſerve, that in meaſuring ſmall angles the triangle need not be perfectly rectangular: the object may ſenſibly vary from the perpendicular, without a perceptible difference in the angle.

QUESTION I.

Mode of determining by the micrometer the diſtance of an object of known magnitude.

The magnitude of an object being known, to determine its diſtance.

For this purpoſe the two images of the object produced by the priſms are to be brought into contact, by moving the index along the axis that carries them. This rule has two diviſions, one ſhewing the minutes and ſeconds, the other the proportion between the diameter of the object and the diſtance. Suppoſe the object I look at is a man: the mean height of a man with his hat on may be eſtimated at ſix feet. Having diſpoſed the priſms and teleſcope ſo that the two images are in one vertical line, I move the index till the feet of the upper image come into contact with the ſummit of the lower: if now the index point at the number 344, I conclude that the diſtance of the man from my eye is 344 times ſix feet, or 344 toiſes. The ſecond diviſion will ſhew, that the angle under which I ſee the man is 10′. By this method the meaſure of a diſtance may unqueſtionably be obtained with great preciſion, but there are many caſes, in which this great preciſion is not neceſſary.

Ships, buildings in which certain rules little liable to variation are obſerved, and the dimenſions of different animals, may ſerve to give this diſtance. By means of ſuch objects I have often determined inacceſſible diſtances with truly ſurpriſing celerity, by means of a portable teleſcope with priſms, which requires no ſupport for taking angles. When greater accuracy is required, objects of which the diameter is well aſcertained muſt be employed.

If lighthouſes had on them a croſs of fixed and known dimenſions, it would thus be of great ſervice to navigators: for by the angle of the perpendicular piece, given by this micrometer, the diſtance of the ſhip from the lighthouſe would be aſcertained; and by that of the horizontal piece would be known the poſition of the ſhip with reſpect to the coaſt.

Croſſes of known dimenſions on light-houſes would be of great utility to navigation.

QUESTION II.

The diſtance of an object being given, to determine its magnitude.

This is preciſely the reverſe of the preceding.

The magnitude of an object determined from the diſtance known.

QUESTION III.

The magnitude and diſtance of an object being unknown, to determine both.

Suppoſe the object makes an angle of 40′ with my eye; as this angle will increaſe in regular proportion as the diſtance is diminiſhed, I approach the object till the angle is 41′, a difference which will be ſufficient in ordinary caſes. If I find the diſtance between the two ſtations, on meaſuring it, to be a hundred toiſes, I ſhall obtain the whole diſtance by multiplying this by the number of minutes of the larger angle, and dividing the product by the difference between the two angles, which will give in this inſtance 4100 toiſes. The diſtance being thus obtained, the magnitude of the object will be found by dividing 4100 toiſes by 83½, the number anſwering to 41′; which will give the magnitude forty-nine toiſes ſeven inches nearly.

Method of determining the magnitude and diſtance, where both are unknown.

XI.

Obſervations on the two lately diſcovered celeſtial Bodies. By WILLIAM HERSCHEL, *LL. D. F. R. S.*

The moving ſtar of Piazzi is very minute.

IN my early account of the moving ſtar diſcovered by Mr. Piazzi, I have already ſhewn that it is of a remarkably ſmall ſize, deviating much from that of all the primary planets *.

It was not my intention to reſt ſatisfied with an eſtimation of the diameter of this curious object, obtained by comparing it with the Georgian planet, and, having now been very ſucceſsful in the application of the lucid diſk micrometer, I ſhall relate the reſult of my inveſtigations.

Another diſcovered by Dr. Olbers.

But the very intereſting diſcovery of Dr. Olbers having introduced another moving ſtar to our knowledge, I have extended my reſearches to the magnitude, and phyſical conſtruction, of that alſo. Its very particular nature, which, from the obſervations I ſhall relate, appears to be rather cometary than planetary, will poſſibly throw alſo conſiderable light upon the circumſtances belonging to the other celeſtial body; and, by that means, enable us to form ſome judgment of the nature of both the two laſt diſcovered phenomena.

As the meaſures I have taken will oblige me to give a reſult which muſt appear extraordinary, it will be highly neceſſary to be particular in the circumſtances of theſe meaſures, and to mention the condition and powers of the teleſcopes that were uſed to obtain them.

Magnitude of the new Stars.

Obſervations on *Ceres* compared with a lucid diſk.

7-feet reflector.

April 1, 1802. Having placed a lucid diſk at a conſiderable diſtance from the eye, but ſo that I might view it with perfect diſtinctneſs, I threw the image of Mr. Piazzi's ſtar, ſeen in a 7-feet reflector, very near it, in order to have the projected picture of the ſtar and the lucid diſk ſide by ſide, that I might aſcertain their comparative magnitudes. I ſoon perceived that the length of my garden would not allow me to remove the diſk-micrometer, which muſt be placed at right angles to the teleſcope, far enough to make it appear no larger than the ſtar; and, not having diſks of a leſs diameter prepared, I placed the ſmalleſt I had as far from me as the ſituation of the ſtar would allow. Then, bringing its image again by the ſide of the diſk, and viewing, at the ſame time, with one eye the magnified ſtar, while the other eye ſaw the lucid diſk, I perceived that Ceres, which is the name the diſcoverer has given to the ſtar, was hardly more than one third of the diameter of the diſk, and certainly leſs than one half of it.

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* By comparing its apparent diſk with that of the Georgian planet, it was eſtimated, that the real diameter of this new ſtar could not amount to three-eighths of that of our moon.

This being repeated, and always appearing the ſame, we ſhall not under-rate the ſize of the ſtar, by admitting its diameter to have been 45 hundredths of the lucid diſk.

Power 370,42.

The power of the teleſcope, very preciſely aſcertained by terreſtrial geometrical meaſures properly reduced to the focus of the mirror on the ſtars, was 370,42. The diſtance of the lucid diſk from the eye, was 2131 inches; and its diameter 3,4 inches. Hence we compute, that the diſk was ſeen under an angle of 5′ 29″,09; and Ceres, when magnified 370 times, appearing, as we have ſhewn, 45 hundredths of that magnitude, its real diameter could not exceed 0″,40. Had this diameter amounted to as much as was formerly eſtimated, the power of 370 would have made it appear of 6′ 10″, which is more than the whole lucid diſk.

Diameter of Ceres 0″,40.

This extraordinary reſult raiſed in me a ſuſpicion, that the power 370 of a 7-feet teleſcope, and its aperture of 6,3 inches, might not be ſufficient to ſhew the planet's feeble light properly. I therefore adapted my 10-feet inſtrument to obſervations with lucid diſks; which require a different arrangement of the head of the teleſcope and finder: I alſo made ſome ſmall tranſparencies, to repreſent the object I intended to meaſure.

2. Obſervation repeated on Ceres with 10-feet reflector. Power 516,54.

April 21. The night being pretty clear, though perhaps not quite ſo proper for delicate viſion as I could have wiſhed, I directed my 10-feet reflector, with a magnifying power of 516,54, alſo aſcertained by geometrical terreſtrial meaſures reduced to the focus of the inſtrument on celeſtial objects, to Mr. Piazzi's ſtar, and compared it with a lucid diſk, placed at 1486 inches from the eye, and of 1,4 inch in diameter. I varied the diſtance of the lucid diſk many times; and fixed at laſt on the above-mentioned one, as the beſt I could find. There was, however, Hazineſs.

however, a hazineſs about the ſtar, which reſembled a faint coma; and this, it may be ſuppoſed, muſt render the meaſure leſs ſatisfactory than it would otherwiſe have been.

Diameter of Ceres 0″,38.

From theſe data we compute, that the diſk appeared to the natural eye under an angle of 3′ 14″,33; while Ceres, when magnified 516½ times, was ſeen by the other eye of an equal magnitude; and that conſequently its real diameter, by meaſurement, was only 0″,38.

3. Obſervation repeated on Ceres; 10-feet reflector, new ſmall mirror;

April 22. 11^{h} 38′, ſidereal time. I uſed now a more perfect ſmall mirror; the former one having been injured by long continued ſolar obſervations. This gave me the apparent diameters of the ſtars uncommonly well defined; to which, perhaps, the very favourable and undiſturbed clearneſs of the atmoſphere might contribute conſiderably.

power 881,51;

With a magnifying power of 881,51, properly aſcertained, like thoſe which have been mentioned before, I viewed Dr. Olbers's ſtar, and compared it with a lucid diſk of 1,4 inch in diameter, placed at 1514 inches from the eye, meaſured, like the reſt of the diſtances, with long deal rods. The ſtar appeared to me ſo ill defined, that, aſcribing it to the eye-glaſs, I thought it not adviſeable to compare the object, as it then appeared, with a well defined lucid diſk. Exchanging the glaſs for that which gives the teleſcope a magnifying power of 516½, I found Pallas, as the diſcoverer wiſhes to have it called, better defined; and ſaw, when brought together, that it was conſiderably leſs in diameter than the lucid diſk.

bad obſ.

Power changed to 516½.

Pallas,

In order to produce an equality, I removed the diſk to 1942 inches; and ſtill found Pallas conſiderably leſs than the diſk.

ſmaller than Ceres.

Before I changed the diſtance again, I wiſhed to aſcertain whether Ceres or Pallas would appear under the largeſt angle, eſpecially as the air was now more pure than laſt night. On comparing the diameter of Ceres with that of the lucid diſk, I found it certainly leſs than the diſk. By proper attention, and continued examination, for at leaſt an hour, I judged it to be nearly ¼ of the lucid diſk.

Then, if we calculate as before, it appears by this obſervation, in which there is great reaſon to place confidence, that the angle under which this ſtar appeared, was only 0″.22. For, a lucid diſk of 1,4 inch diameter, at the diſtance of 1942 inches, would be ſeen under an angle of 2′ 28″,7; three quarters of which are 1′ 51″,52. This quantity, divided by the power 516,54, gives 0″,2159, or, as we have given it abridged, 0″,22.

Excellent reſult of Ceres, diam. 0″,22.

13^h 7′. I removed the micrometer to the greateſt convenient diſtance, namely, 2136 inches, and compared Dr. Olbers's ſtar, which, on account of its great altitude, I ſaw now in high perfection, with the lucid diſk. It was, even at this diſtance, leſs than the diameter of the diſk, in the proportion of 2 to 3.

When, by long continued attention, the appearance of Pallas was reduced to its ſmalleſt ſize, I judged it to bear no greater proportion to the diameter of the lucid diſk of the micrometer, than as 1 to 2.

In conſequence of theſe meaſures, it appears that the diameter of Pallas, according to the firſt of them, is 0″,17; and, according to the laſt, where the greateſt poſſible diſtinctneſs was obtained, only 0″,13.

Diam. of Pallas 0″,13.

If it ſhould appear almoſt incredible that theſe curious objects could give ſo ſmall an image, had they been ſo much magnified as has been reported, I can ſay, that curioſity led me to throw the picture of Jupiter, given by the ſame teleſcope and magnifying power, on a wall at the diſtance of 1318 inches, of which it covered a ſpace that meaſured 12 feet 11 inches. I do not mention this as a meaſure of Jupiter, for the wall was not perfectly at right angles to the teleſcope, on which account the projected image would be a little larger than it ſhould have been, nor was I very attentive to other neceſſary minute circumſtances, which would be required for an accurate meaſure; but we ſee at once, from the ſize of this picture, that the power of the teleſcope exerted itſelf to the full of what has been ſtated.

Proof by the image of Jupiter that the power of the teleſcope was truly as here ſtated.

As we generally can judge beſt of comparative magnitudes, when the meaſures are, as it were, brought home to us; it will not be amiſs to reduce them to miles. This, however, cannot be done with great preciſion, till we are more perfectly acquainted with the elements of the orbits of theſe ſtars. But, for our preſent purpoſe, it will be ſufficiently accurate, if we admit their mean diſtances from the ſun, as the moſt recent information at preſent ſtates them; for Ceres 2,6024; and for Pallas 2,8. The geocentric longitudes and north latitudes, at the time of

Rough computation of the diameters of theſe bodies in miles;

of obſervation, were, for Ceres, about ♍ 20° 4′, 15° 20′; and for Pallas, ♍ 23° 40′, 17° 30′, With theſe data, I have calculated the diſtances of the ſtars from the earth at the time of obſervation, partly by the uſual method, and, where the elements were wanting, by a graphical proceſs, which is ſufficiently accurate for our purpoſe. My computed diſtances were 1,634 for Ceres, and 1,8333 for Pallas; and, by them we find, that the diameter of Ceres, at the mean diſtance of the earth from the ſun, would ſubtend an angle of 0″,35127; and that, conſequently, its real diameter is 161,6 miles.

That of Ceres proves to be 161,6 miles; and that of Pallas 147 or 110$\frac{1}{2}$ miles.

It alſo follows, that Pallas would be ſeen, at the ſame diſtance from the ſun, under an angle of 0″,3199; and that its real diameter, if the largeſt meaſure be taken, is 147 miles; but, if we take the moſt diſtinct obſervation, which gives the ſmalleſt meaſure, the angle under which it would be ſeen from the ſun, will be only 0″,2399; and its diameter, no more than 110$\frac{1}{3}$ miles.

Of Satellites.

Examination for diſcovering Satellites.

None were aſcertained.

After what has juſt now been ſhewn, with regard to the ſize of theſe new ſtars, there can be no great reaſon to expect that they ſhould have any ſatellites. The little quantity of matter they contain, would hardly be adequate to the retention of a ſecondary body; but, as I have made many obſervations with a view to aſcertain this point, it will not be amiſs to relate them.

Feb. 25. 20-feet reflector. There is no ſmall ſtar near Ceres, that could be ſuppoſed to be a ſatellite.

Feb. 28. There is no ſmall ſtar within 3 or 4 minutes of Ceres, that might be taken for a ſatellite.

March 4. 9^{h}. 45′. ſidereal time. A very ſmall ſtar, ſouth-preceding Ceres, may be a ſatellite. See Plate V. Fig. 1. where C is Ceres, S the ſuppoſed ſatellite, *a b c d e f*, are delineation ſtars, *c* and *d* are very ſmall. S makes nearly a right angle with them; *e* is larger than either *c* or *d*. There is an extremely faint ſtar *f*, between *e* and *d*.

14^{h}. 16′. Ceres has left the ſuppoſed ſatellite behind.

March 5. There are two very ſmall ſtars, which may be ſatellites; ſee Fig. 2. where they are marked, 1ſt S, 2d S. The reſt, as before, are delineation ſtars.

March 6. The two ſuppoſed ſatellites of laſt night remain in their ſituation, Ceres having left them far behind.

10^{h}. 16′. There is a very ſmall ſtar, like a ſatellite, about 75° ſouth-following Ceres. See Fig. 3. It is in a line from C to *b* of laſt night.

11_{h}. 20′. Ceres has advanced in its orbit; but has left the ſuppoſed ſatellite behind.

March 30. 9^{h}. 35′. A ſuppoſed 1ſt ſatellite is directly following Ceres: it is extremely faint. A 2d ſuppoſed ſatellite is north-following. See Fig. 4. The ſuppoſed ſatellites are ſo ſmall, that, with a 20-feet teleſcope, they require a power of 300 to be ſeen; and the planet ſhould be hidden behind a thick wire, placed a little out of the middle of the field of view, which muſt be left open to look for the ſuppoſed ſatellites.

12^{h} 17′. Ceres has changed its place, and left both the ſuppoſed ſatellites behind.

March 31. 9^{h} 20′. There is a very ſmall ſtar, on the north-preceding ſide of Ceres, which may be a ſatellite.

11^{h} 50′. Ceres has moved forwards in its path; but the ſuppoſed ſatellite remains in its former ſituation. The neareſt ſtar is 20″ of time from Ceres; ſo that, within a circle of 40″ of time, there certainly is no ſatellite that can be ſeen with the ſpace-penetrating power of this inſtrument.

It is evident, that when the motion of a celeſtial body is ſo conſiderable, we need never be long in doubt whether a ſmall ſtar be a ſatellite belonging to it, ſince a few hours muſt decide it.

May 1. 12^{h} 51′. I viewed Pallas with the 20-feet reflector, power 300; there was no ſtar within 3′, that could be taken for a ſatellite.

Of the Colour of the new Stars.

Feb. 13. The colour of Ceres is ruddy, but not very deep.
April 21. Ceres is much more ruddy than Pallas.
April 22. Pallas is of a duſky whitiſh colour.

Ceres is of a ruddy colour; Pallas duſky white.

Of the Appearances of the new Stars, with regard to a Diſk.

Feb. 7. Ceres, with a magnifying power of 516½, ſhews an ill defined planetary diſk, hardly to be diſtinguiſhed from the ſurrounding hazineſs.

The diſk of Ceres is ill-defined;

Feb. 13. Ceres has a viſible diſk.

April 22. In viewing Pallas, I cannot, with the utmoſt attention, and under the moſt favourable preſent circumſtances, perceive any ſharp termination which might denote a diſk; it is rather what I would call a nucleus.

Pallas is not a diſk but a nucleus.

April 28. In the finder, Pallas is leſs than Ceres. It is alſo rather leſs than when I firſt ſaw it.

Of the Appearances of the new Stars, with regard to an Atmoſphere, or Coma.

Ceres has moſt frequently a coma (perhaps effected by the inſtrument, or more probably by the atmoſphere.)

April 21. I viewed Ceres for nearly an hour together. There was a hazineſs about it, reſembling a faint coma, which was, however, eaſily to be diſtinguiſhed from the body.

April 22. I ſee the diſk of Ceres better defined, and ſmaller than I did laſt night. There does not ſeem to be any coma; and I am inclined to aſcribe the appearance of laſt night to a deception, as I now and then, with long attention, ſaw it without; at which times it was always beſt defined, and ſmalleſt.

April 28. Ceres is ſurrounded with a ſtrong hazineſs. Power 550.

With 516½, which is a better glaſs, the breadth of the coma beyond the diſk may amount to the extent of a diameter of the diſk, which is not very ſharply defined. Were the whole coma and ſtar taken together, they would be at leaſt three times as large as my meaſure of the ſtar. The coma is very denſe near the nucleus; but loſes itſelf pretty abruptly on the outſide, though a gradual diminution is ſtill very perceptible.

April 30. Ceres has a viſible, but very ſmall coma about it. This cannot be ſeen with low powers; as the whole of it together is not large enough, unleſs much magnified, to make up a viſible quantity.

May 1. The diameter of the coma of Ceres, is about 5 times as large as the diſk, or extends nearly 2 diameters beyond it.

13ʰ 19′. 20-feet reflector; power 477. The diſk of Ceres is much better defined than that of Pallas. The coma about it is conſiderable, but not quite ſo extended as that of Pallas.

May 2. 13ʰ 20′. Ceres is better defined than I have generally ſeen it. Its diſk is ſtrongly marked; and, when I ſee it beſt, the hazineſs about it hardly exceeds that of the ſtars of an equal ſize.

Memorandum. This may be owing to a particular diſpoſition of the atmoſphere, which ſhews all the ſtars without twinkling, but not quite ſo bright as they appear at other times. Jupiter likewiſe has an extremely faint ſcattered light about it, which extends to nearly 4 or 5 degrees in diameter.

April 22. Pallas, with a power of 881½, appears to be very ill defined. The glaſs is not in fault; for, in the day time, I can read with it the ſmalleſt letters on a meſſage card, fixed up at a great diſtance.

Pallas is ill-defined, nebulous or cometary.

13h 17′. The appearance of Pallas is cometary; the diſk, if it has any, being ill defined. When I ſee it to the beſt advantage, it appears like a much compreſſed, extremely ſmall, but ill defined, planetary nebula.

April 28. Pallas is very ill defined: no determined diſk can be ſeen. The coma about it, or rather the coma itſelf, for no ſtar appears within it, would certainly meaſure, at firſt ſight, 4 or 5 times as much as it will do after it has been properly kept in view, in order to diſtinguiſh between the hazineſs which ſurrounds it, and that part which may be called the body.

May 1. Pallas has a very ill defined appearance; but the whole coma is compreſſed into a very ſmall compaſs.

13h 5′. 20-feet reflector; power 477. I ſee Pallas well, and perceive a very ſmall diſk, with a coma of ſome extent about it, the whole diameter of which may amount to 6 or 7 times that of the diſk alone.

very ſmall diſk.

May 2. 13h 0′. 10-feet reflector. A ſtar of exactly the ſame ſize, in the finder, with Pallas, viewed with 516½, has a different appearance. In the centre of it is a round lucid point, which is not viſible in Pallas. The evening is uncommonly calm and beautiful. I ſee Pallas better defined than I have ſeen it before. The coma is contracted into a very narrow compaſs; ſo that perhaps it is little more than the common aberration of light of every ſmall ſtar. See the memorandum to the obſervations of Ceres, May 2.

On the Nature of the new Stars.

From the account which we have now before us, a very important queſtion will ariſe, which is, What are theſe new ſtars, are they planets, or are they comets? And, before we can

Nature of the new ſtars.

can enter into a proper examination of the ſubject, it will be neceſſary to lay down ſome definition of the meaning we have hitherto affixed to the term planet. This cannot be difficult, ſince we have ſeven patterns to adjuſt our definition by. I ſhould, for inſtance, ſay of planets,

Criterions of planets.

1. Magnitude.

1. They are celeſtial bodies, of a certain very conſiderable ſize.

2. Orbits nearly circular.

2. They move in not very excentric ellipſes round the ſun.

3. Nearly in the pl. ecliptic.

3. The planes of their orbits do not deviate many degrees from the plane of the earth's orbit.

4. Motion direct.

4. Their motion is direct.

5. Satellites or rings.

5. They may have ſatellites, or rings.

6. Atmoſphere, not extenſive.

6. They have an atmoſphere of conſiderable extent, which however bears hardly any ſenſible proportion to their diameters.

7. Orbits conſiderably aſunder.

7. Their orbits are at certain conſiderable diſtances from each other.

Compariſon of the new ſtars in theſe reſpects.

Now, if we may judge of theſe new ſtars by our firſt criterion, which is their ſize, we certainly cannot claſs them in the liſt of planets: for, to conclude from the meaſures I have taken, Mercury, which is the ſmalleſt, if divided, would make up more than 135 thouſand ſuch bodies as that of Pallas, in bulk.

In the ſecond article, their motion; they agree perhaps ſufficiently well.

The third, which relates to the ſituation of their orbits, ſeems again to point out a conſiderable difference. The geocentric latitude of Pallas, at preſent, is not leſs than between 17 and 18 degrees; and that of Ceres between 15 and 16; whereas, that of the planets does not amount to one half of that quantity. If bodies of this kind were to be admitted into the order of planets, we ſhould be obliged to give up the zodiac; for, by extending it to them, ſhould a few more of theſe ſtars be diſcovered, ſtill farther and farther deviating from the path of the earth, which is not unlikely, we might ſoon be obliged to convert the whole firmament into zodiac; that is to ſay, we ſhould have none left.

In the fourth article, which points out the direction of the motion, theſe ſtars agree with the planets.

(To be continued in our next.)

A

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AND

THE ARTS.

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ARTICLE I.

Comparative Experiments and Obſervations on Myrtle Wax, Bees Wax, Spermaceti, Adipocire, and the Cryſtalline Matter of biliary Calculi. In a Letter from John Bostock, *M. D.*

To Mr. NICHOLSON.

Liverpool, Jan. 23, 1803.

SIR,

Enquiry concerning the nature and uſes of myrtle wax.

IN the courſe of laſt ſummer, I was requeſted by a friend, who had received from America a quantity of the peculiar vegetable matter called Myrtle Wax, to give him ſome information reſpecting its nature and properties, and the probable uſe to which it might be advantageouſly applied. For this purpoſe I examined ſeveral works on natural hiſtory and chemiſtry, but I could only meet with a general account of the method employed for obtaining it, with a vague deſcription of its phyſical properties. Reſpecting its chemical analyſis I could find nothing ſatisfactory; even the elaborate work of Fourcroy contains little preciſe information on this head [*]. I

Scarcely mentioned by Fourcroy.

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[*] Fourcroy, Syſtème des Connoiſſances Chimiques, tom. vii. p. 339.

determined therefore to examine this point more accurately, and with this intention, the experiments of which I ſend you an abſtract were performed. Some other ſubjects however having at that time occupied me, I had almoſt forgotten my analyſis of the myrtle wax, until in looking over the 131ſt No. of the *Annales de Chimie,* I found that the ſubject had been treated in an ample manner by M. Cadet. This circumſtance induced me to recur to my Papers, in order to compare my experiments with thoſe of M. Cadet; when I found that though his memoir contains a very complete account of the natural hiſtory of the ſubſtance, yet that my chemical analyſis was more minute. On this account I conceive, that if you have any intention of inſerting in your Journal * a tranſlation of M. Cadet's eſſay, my communication may prove a uſeful ſupplement.

But more fully by Cadet.

I am,
Your obedient ſervant,
JOHN BOSTOCK.

Deſcription and Analyſis of Myrtle Wax.

Obvious properties of myrtle wax.

The peculiar ſubſtance obtained from the Myrica Cerifera of Linnæus †, and which has obtained the name of myrtle wax, is a concrete body, of moderate hardneſs and conſiſtence; it has in part the tenacity of bees wax, though without its unctuoſity; along with this, it alſo poſſeſſes, in ſome degree, the brittleneſs of the reſins. The colour of the myrtle wax is a pale green; the ſhades of the different pieces are however ſomewhat varied; in moſt of them, the green has a tendency to a dirty grey; in others, it is lighter, more tranſparent, and of a yellowiſh tinge. Its ſpecific gravity is about 1,0150, water being 1,0000, and white bees wax, 9600. It is fuſed at a temperature of 109°; by ſufficiently increaſing the heat, it burns with a peculiarly clear, white flame, produces little ſmoke, and during the combuſtion emits an agreeable aromatic odor.

Habitudes with reagents. Water has no action.

The following are its habitudes with the different re-agents.

1. Water has no action upon the myrtle wax, either when cold or at the boiling heat.

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* The reader will find it in the preſent Number. N.

† Martyn's edition of Millar's Dictionary.

Alcohol,

2. Alcohol, at the ordinary temperature of the atmoſphere, has no action upon it; but 100 parts, by weight, of this fluid, when boiling, diſſolve about five parts of the wax. Nearly $\frac{4}{5}$ is depoſited by the cooling of the alcohol; $\frac{1}{5}$ ſtill remains ſuſpended; this is ſlowly depoſited in the courſe of a few days, or may be precipitated by the addition of water. This ſubſtance, when precipitated from the alcohol, is lighter coloured than in its original ſtate, and approaches more to a grey tinge. Though the myrtle wax ſeems to be homogeneous in its texture and conſiſtence, it is not totally ſoluble in alcohol: about $\frac{4}{5}$ only of the whole is acted upon by this fluid, even when boiling. The part which is inſoluble in alcohol, when ſeparated from the reſt of the maſs, exhibits a ſomewhat darker ſhade of green than before the experiment; the alcohol remains completely colourleſs.

Cold alcohol does not act upon it. Hot alcohol ſparingly diſſolves it in part only, and depoſits this by cooling and repoſe.

3. Sulphuric ether, when at the common temperature of the atmoſphere, diſſolves the myrtle wax only in ſmall quantity, but it acts upon it rapidly when boiling. On account of the tendency which the fluid has to evaporate, it is difficult to aſcertain the exact proportion, but it ſeems to take up ſomewhat more than $\frac{1}{4}$ of its own weight. The greateſt part of this is ſeparated as the ether cools, and the reſt may be precipitated by the addition of water. The wax, after it has been diſſolved in ether, is nearly colourleſs, while the fluid aſſumes a beautiful green hue. If the wax be not too abundant, and the ether be ſuffered to evaporate ſlowly, the wax is depoſited on the ſides of the glaſs in a cryſtalline or lamellated form; in this ſtate its texture approaches ſomewhat to that of ſpermaceti.

Cold S. Ether diſſolves little; hot ether more rapidly and largely. The depoſit by evaporation reſembles ſpermaceti.

4. Rectified oil of turpentine at the temperature of the atmoſphere ſoftens the wax, but does not ſeem capable of diſſolving it; when aſſiſted by a moderate heat, 100 grains of the turpentine diſſolve ſix grains of the myrtle wax. The turpentine acquires a light green tinge, part of the wax is ſeparated as the fluid cools, while part remains permanently diſſolved in it.

Ol. Turpentine acts but ſlightly.

5. When the myrtle wax is boiled with liquid cauſtic potaſh, the fluid becomes turbid, but after ſome time the greateſt part of the wax riſes to the ſurface, nearly without colour, in a flocculent form. A ſmall quantity of it only remains diſ-

Pure potaſh in water renders it colourleſs by boiling, and forms a ſoap with a ſmall part, which being decompoſed by acid affords the wax nearly unchanged.

ſolved in the potaſh; this may be precipitated from it by an acid. That part of the wax which riſes to the ſurface is converted into a ſaponaceous matter; it has loſt its inflammability and fuſibility, and forms an opake ſolution with water. From this ſolution it is precipitated by an acid in the form of white flakes, which when collected, will be found to reſemble very nearly the wax before its union with the potaſh.

Pure ammonia acts nearly as potaſh, but more feebly.

6. Pure ammonia exhibits with the myrtle wax phenomena in many reſpects, ſimilar to thoſe produced by the fixed alcalis. When its action is promoted by heat, an opake ſolution is produced; the wax is deprived of its colour; the greateſt part of it ſeparates from the fluid, and is converted into a ſubſtance partly ſoluble in warm water, though leſs ſo than that reſulting from the action of potaſh upon myrtle wax.

The three principal mineral acids act upon it; though not with any notable force.

7. The mineral acids exerciſe little action upon this ſubſtance at the ordinary temperature of the atmoſphere; the ſulphuric diſſolves it ſparingly, and acquires a brown tinge. With the aſſiſtance of a moderate heat this acid diſſolves about $\frac{1}{12}$ of its weight, and is converted into a thick, dark-brown maſs; by cooling it becomes nearly concrete, but no ſeparation of the wax takes place. The nitric and muriatic acids, even when heated, ſeem to poſſeſs little attraction for the myrtle wax. After the wax had been kept for ſome time in a ſtate of fuſion in contact with the nitric acid, its green hue was converted into a pale yellow, but the acid remained colourleſs, nor did any part of the wax appear to be diſſolved in it: the wax by long digeſtion in muriatic acid became of a bright orange colour.

Deduction: that myrtle wax appears to be an oxigenated fixed oil.

Theſe experiments will enable us, at leaſt with a conſiderable degree of probability, to aſſign the place which the vegeable myrtle wax muſt hold in a natural arrangement of chemical ſubſtances. Its inflammability, fuſibility, its inſolubility in water, and the action which takes place between it and the alcalis, point out its affinity to the fixed oils, while its texture and conſiſtence, and more particularly its habitudes with alcohol and ether, indicate a reſemblance to the reſins. We may therefore conſider the myrtle wax as a fixed vegetable oil, rendered concrete by the addition of a quantity of oxigen; it ſeems to hold the ſame relation to the fixed, that reſins do to the eſſential oils of vegetables.

But it reſembles the wax of the bee; and alſo ſpermaceti; and the adipocire produced by nitric acid upon muſcular fibre; and the cryſtalline matter of biliary calculi.

But though the myrtle wax be itſelf of vegetable origin, there are ſome animal ſubſtances which more nearly reſemble it in its chemical properties than any product of the vegetable kingdom. The principal of theſe is the wax elaborated by the bee, to which the peculiar ſubſtance now under conſideration bears a ſtrong reſemblance, both in its phyſical and chemical properties. Myrtle wax alſo in many particulars reſembles ſpermaceti; the ſubſtance called Adipocire, produced by the action of nitric acid upon the muſcular fibre; and the cryſtalline matter of biliary calculi. I ſhall in a very conciſe manner, inſtitute a compariſon between myrtle wax and each of theſe ſubſtances.

Bees Wax.

Compariſon with bees wax. Myrtle wax is more unctuous; tenacious; different in colour and ſmell; more fuſible, more ſoluble in alcohol and in ether; more affected by potaſh, and nearly the ſame by ammonia.

This ſubſtance, in its phyſical properties, differs from myrtle wax in being more unctuous, and poſſeſſing a greater degree of tenacity: its colour and ſmell are alſo different. Bees wax is likewiſe conſiderably leſs fuſible; Dr. Pearſon * and Mr. Nicholſon † fix its melting point at 142°; whereas Mr. Fourcroy ‡ places it lower in the ſcale at 117°; on this ſubject the reſults of my experiments coincide exactly with thoſe of the Engliſh chemiſts. There is alſo a difference of opinion among chemiſts reſpecting the action which takes place between this ſubſtance and alcohol; Fourcroy §, Chaptal ||, and Nicholſon **, aſſert that it is inſoluble in this fluid, while Pearſon maintains the contrary; on this queſtion my experience agrees with that of Dr. Pearſon. The proportion of bees wax which the alcohol is capable of diſſolving, ſeems however to be ſomewhat leſs than that of the myrtle wax. As in the former caſe, the greateſt part of the wax ſeparates as the fluid cools; while the remainder may be precipitated by the addition of water. Bees wax is ſparingly diſſolved by boiling ether, leſs readily, and in conſiderably leſs proportion

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* Pearſon's Obſervations and Experiments on White Lac. Phil. Tranſ. 1794.

† Nicholſon's Journal, Quarto, vol. i. p. 70.

‡ Fourcroy Syſtème des Connaiſſ. Chimiques, x. 343.

§ Ibid. and Thomſon's Fourcroy, Vol. iii. p. 387.

|| Chaptal's Elements, Vol. III. p. 164.

** (Nicholſon's Elements, p. 502.)

than the myrtle wax; this fluid, when heated, ſeems only to take up about $\frac{1}{20}$ of its weight of bees wax. Cauſtic potaſh exhibits the ſame phenomena with bees wax as with the product of the myrica cerifera; it was converted into the ſaponaceous ſtate, and became ſoluble in warm water. It appeared however that the action was leſs violent, and the change leſs complete than in the former caſe. Ammoniac, when boiling, readily forms with bees wax an emulſion, in ſome reſpects reſembling that produced by the ſame ſubſtance with the myrtle wax. As the mixture cools, the greateſt part of the wax riſes to the ſurface in a flocculent form; it appears to have ſo far contracted a union with the alcali as to have its texture and odour deſtroyed, and its fuſibility and inflammability diminiſhed; yet it is little, if at all, ſoluble in water.

Spermaceti.

Spermaceti: Is more fuſible than either of the preceding.

Sparingly ſoluble in alcohol,

rapidly by hot ether,

and oil of turpentine.

More ſtrongly attacked by potaſh than m. or b. wax.

One property of this ſubſtance, which obviouſly diſtinguiſhes it from thoſe already deſcribed, is the cryſtalline texture which it conſtantly aſſumes. It is more fuſible than either of the ſubſtances which we have examined; but reſpecting the preciſe temperature at which it becomes liquid, there is a conſiderable difference of opinion. Fourcroy * ſtates it to be at the 98th degree, or a little lower †; Mr. Nicholſon ‡ ſuppoſes it to be at the 133d degree; I have found the melting point of ſpermaceti to be uniformly 112°; there may perhaps be a real difference in the ſpecimens that have been employed. Like the two kinds of wax, it is ſoluble in alcohol, though very ſparingly; according to my experiments, it required a quantity of boiling alcohol equal to 150 times the weight of the ſpermaceti to diſſolve it; a proportion which nearly coincides with the eſtimate of Fourcroy: the whole is precipitated as the fluid cools. It is rapidly diſſolved by warm ether, by cooling it is precipitated ſo plentifully, as in appearance to convert the whole into a ſolid cryſtallized maſs. Spermaceti is alſo diſſolved with great facility by oil of turpentine gently heated, but is depoſited from it as it cools. It unites very readily with cauſtic potaſh, and the compound is completely ſoluble in warm water: potaſh ſeems to exerciſe upon ſpermaceti a more powerful action, than upon either the myrtle or the bees wax. Ammoniac, at the uſual temperature of the atmoſphere, does not appear to exerciſe any action upon ſpermaceti, but when boiling it unites with it readily, and forms an emulſion, which is not decompoſed by the cooling of the mixture, or by the addition of water; but the ſpermaceti is inſtantly precipitated by the addition of an acid. No important phenomena reſult from the action of the mineral acids upon ſpermaceti.

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* Encyc. Math. Chimie, Art. Blanc de Baleine.

† Annales de Chim. tom. vii. 192.

‡ Nicholſon's Journal, Vol. I. 4to, p. 70.

Strongly by hot ammonia, and then not decompoſ. by cooling or dilution, but by an acid.

Adipocire.

Adipocire. Deſcription.

I procured a quantity of this ſubſtance by digeſting diluted nitric acid upon the muſcular fibre; it was afterwards waſhed in warm water, in order to ſeparate any portion of adhering acid. The matter thus purified, was of a light yellow colour, of about the conſiſtence of tallow, and of a homogeneous texture. Reſpecting the temperature at which it is fuſed, we meet with the ſame uncertainty as in the former caſes. Fourcroy in one of his eſſays[*] fixes its melting point at the 98th degree; the ſame author in another place ſtates it to be the 110th[†], while Mr. Nicholſon[‡] ſuppoſes it to be as high as the 127th; in Dr. Rees's Cyclop.[§] it is ſtated, that this ſubſtance melts at 7° below ſpermaceti, which according to my eſtimate would be the 105th degree. In my own experience upon this ſubject it became liquid at the 92d degree. Alcohol, at the ordinary temperature of the atmoſphere, diſſolves it only in ſmall quantity, but by the aſſiſtance of a gentle heat it acts upon it with rapidity. Fourcroy[‖] ſtates that this fluid when boiling diſſolves about its own weight of adipocire, $\frac{1}{4}$ or $\frac{1}{5}$ of which is retained after the fluid cools. The ſame chemiſt in another memoir aſſerts, that one ounce of alcohol will diſſolve 12 drams of this ſubſtance[**]. There may probably be ſome difference in the chemical nature of adipocire, according

Fuſibility.

Solubility in hot alcohol, conſiderable.

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[*] Ann, de Chimie, tom. vii. 192.

[†] Ann de Chimie, tom. viii. 66.

[‡] Nicholſon's Journal, ubi ſupra.

[§] Rees's Cyclop. new edit. Art. Adipocire, this is probably inſerted only upon the authority of Fourcroy.

[‖] Ann. de Chimie, VII. 191.

[**] Ann. de Chimie, VIII. 67.

to the proceſs by which it is obtained, or the rapidity of its production; in my experiments the quantity which the alcohol was capable of diſſolving, though very conſiderable, was certainly leſs than that ſtated by Fourcroy. The greater part is depoſited as the fluid cools, and the remainder may be precipitated by water. The adipocire, after this operation, is rendered nearly white, while the alcohol aſſumes a deep yellow tinge. Ether diſſolves it ſparingly, when unaſſiſted by heat; when boiling it takes up about ¼ of its own weight; this is for the moſt part ſeparated by the cooling of the fluid; the adipocire is depoſited nearly white, while the ether acquires a yellowiſh green colour. The cauſtic alcalis, both fixed and volatile, exert upon this ſubſtance the ſame kind of action which we have deſcribed in the former inſtances; when heated in contact with it, they form a ſaponaceous emulſion of a reddiſh brown colour, which is miſcible with water without decompoſition. The volatile alcali diſſolves it ſparingly, without the aſſiſtance of heat; a circumſtance in which the adipocire differs both from any of the ſubſtances which we have hitherto examined, and alſo from the cryſtalline matter of biliary calculi. Upon the whole the adipocire is more fuſible, more inflammable, and more eaſily acted upon by the different reagents than any ſubſtance which has paſſed under our review.

and in ether.

Alkalis as before.

Ammonia diſſolves it cold.

Cryſtalline Matter of Biliary Calculi.

Cryſtals of biliary calculi. Deſcription.

It now only remained to perform ſome comparative experiments with the cryſtalline matter of biliary calculi, and I was fortunately in poſſeſſion of two of theſe bodies, which were preſented to me by Dr. Gerard of this place. The calculi were ſimilar in their texture and appearance, and of nearly the ſame ſize; the one which I examined was of an irregular, polyhedral figure, with its edges and angles blunted; it was ſomewhat brittle, and of an ochry brown colour. Its ſpecific gravity was about ,9000; it weighed 16 grains. When broken, and viewed through a microſcope, it was found to conſiſt of an internal nucleus, formed of radii converging to a centre, and of an external cruſt compoſed of four or five thin ſtrata. It evidently conſiſted of two diſtinct ſubſtances; one white, ſparkling, and of a cryſtalline texture, by which its general ſtructure was determined; the other a number of dark coloured particles, irregularly diſperſed through the interſtices of the former. I therefore concluded it to be that kind of calculus which Fourcroy calls Cyſtic-Adipobileous [*]; compoſed of the peculiar cryſtallized matter, and of particles of inſpiſſated bile, mixed together in different proportions.

Fuſible and inflammable; not acted on by water.

The cryſtalline matter is fuſible and inflammable, but the preciſe degree at which it melts has not been aſcertained; it is not even ſoftened by the heat of boiling water.

Hot alcohol diſſolves it ſparingly,

Alcohol in the cold has no action upon it, but when boiling it diſſolves it with facility; Fourcroy [†] ſtates, that one part of this ſubſtance is taken up by 19 parts of the fluid; this however was not the caſe with the calculus which I examined; in this inſtance the alcohol certainly did not diſſolve more than $\frac{1}{30}$ of its weight of the cryſtalline matter. As the fluid cools, the ſubſtance is depoſited in the form of white, ſhining ſpiculæ, intermixed with thin plates.

ether more fully.

Ether diſſolves it ſlowly in the cold, but more readily when heated; the greateſt part is depoſited as the fluid cools, and the reſt may be precipitated by water. If the ether be ſuffered to evaporate ſlowly at the ordinary temperature of the atmoſphere, the matter which it held in ſolution will be depoſited on the ſides of the glaſs, in the form of beautiful radiated cryſtals.

Ol. turpentine little.

Oil of turpentine acts upon this matter with difficulty; it appears, however, when digeſted with it for ſome time at the boiling heat, to diſſolve it in a ſmall degree.

Fixed alkalis as before, and ammonia ſcarcely at all.

It is acted upon by cauſtic potaſh when boiling, and the reſult of their union appears to be of the ſame nature with that deſcribed in the former inſtances. A ſmall quantity is diſſolved by the pot-aſh, and may be precipitated by an acid; while another part is converted into a ſubſtance ſoluble in water, but inſoluble in alcohol; it may be precipitated from the water by an acid. Ammoniac, even when boiling, ſeems to poſſeſs little or no attraction for this cryſtalline matter.

The reſults of ſeveral experiments oblige me to differ from Dr. Powel, reſpecting the effect of the fixed alcalis upon this peculiar ſubſtance; he conceives that it is not acted upon by them [‡]; the opinion which I have adopted is however ſupported by the authority of Fourcroy [§].

Nitric acid acts upon and alters it.

Nitric acid, more

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[*] Fourcroy, Syſtême des Conn. Chim. x. 59.

[†] Ann. de Chimie, VII.

[‡] Powel on the Bile, 119.

[§] Encyc. Meth. Chimie; art. Bile; Fourcroy, Syſtême des Conn. Chimiques, x.

particularly when aſſiſted by heat, acts readily upon the cryſtalline matter; during the proceſs there is a diſengagement of nitrous gas. A ſmall quantity remains diſſolved in the fluid, and may be precipitated from it by potaſh. The greateſt part however riſes to the ſurface as the fluid cools in the form of drops of oil, which gradually grow concrete; the cryſtalline texture is deſtroyed, and its conſiſtence reſembles that of a vegetable reſin. Water does not diſſolve this peculiar matter, but it ſeems to render it ſomewhat more brittle and friable. Alcohol aſſiſted by a gentle heat diſſolved it; it was precipitable from the ſolution by water in the form of a grey powder. Ether, at the temperature of the atmoſphere, diſſolved it rapidly; water precipitated it from the ether in the form of drops of oil. When the fluid was evaporated, it was depoſited, without exhibiting any marks of a cryſtalline ſtructure. Cauſtic potaſh acted upon it without the aſſiſtance of heat, when boiling it diſſolved it with more facility; the fluid acquired a reddiſh brown hue; it was not precipitated by water, but the ſulphuric acid ſeparated it in the form of a grey powder. The action of ammoniac was nearly ſimilar, though as I conceived, ſomewhat more powerful than that of the fixed alcalis. The ſolution was alſo of a reddiſh brown colour; but the precipitate by ſulphuric acid was of a bright yellow. It appears therefore that the matter of biliary calculi has its properties materially changed by the operation of the nitric acid. It entirely deſtroys its cryſtalline tendency, and renders it more ſoluble in ether and in the alcalis. It has been ſuppoſed that by this proceſs it becomes more aſſimilated to the adiporeſin of the bile *, but it ſtill differs from it in not poſſeſſing any degree of ſolubility in water †.

Habitudes of the altered ſubſtances.

The ſubſtances here treated are really different.

Theſe remarks upon the cryſtalline matter of biliary calculi, ſhew that there are ſeveral important circumſtances in which it differs both from ſpermaceti, and from adipocire, to both of which it has been compared ‡. Upon the whole, though the

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* Powel, p. 121.

† Ann. de Chimie, tom. vii. p. 178. Encyc. Meth. Chimie, art. Bile, p. 566.

‡ I may obſerve that the dark-coloured particles which were diſperſed through the cryſtals of the calculus, though they might have been originally compoſed of inſpiſſated bile, did not retain the properties of this ſubſtance, as they were nearly inſoluble both in boiling water and in alcohol.

five ſubſtances which have paſſed under our review poſſeſs certain properties in common, and have a degree of ſimilarity in their external appearance, yet they differ materially in their chemical nature. There is indeed reaſon to conjecture that they are all compoſed of the ſame elements, combined together in different proportions and with different degrees of attraction.

General remarks.

Lavoiſier * firſt made us acquainted with the chemical compoſition of oil, and proved that it conſiſts of hydrogene and carbone. This great philoſopher alſo demonſtrated that wax differs from oil, in containing a greater proportion of carbone; there is every reaſon to ſuppoſe, that a quantity of oxygene likewiſe enters into its compoſition. It may be conjectured, that the five ſubſtances which have paſſed under our review, differ from each other in the proportion of oxygene, hydrogene, and carbone; but the preſent ſtate of our knowledge will not enable us to determine how far the individual properties will be affected by the different proportions. It had been conjectured, that an addition of carbone renders a body leſs fuſible, and at the ſame time more ſoluble in alcohol; but we find from the experiments recited above, that adipo-cire, which is the moſt fuſible, is likewiſe the moſt ſoluble in alcohol. Probably a good deal may depend in theſe caſes upon the ſtate of the combination of the ingredients, as well as upon their proportions.

II.

On the Nature of Muſical Sounds. In Reply to DR. YOUNG. *By Mr.* JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

Introductory remark.

THE controverſy between Dr. Young and me has taken a new turn in your laſt number; for my opponent, with the addreſs of an able politician, endeavours to ſecure the victory by the aid of a powerful auxiliary. For this purpoſe, he has attempted to oppreſs my cauſe, and ſilence my reaſons, by the authority of an illuſtrious name; whom he has impreſſed into his ſervice. The only plea that I can offer in oppoſition to this formidable confederacy, conſiſts in the following plain remark: I contend for the truth and not for ſuperiority, a motive which obliges every diſputant to diſregard all authority, except the conviction impreſſed on the ſenſes by a judicious experiment, and the aſſent given by the underſtanding to ſound arguments. Notwithſtanding the preceding declaration, I acknowledge the weight of perſonal reſpectability, in many inſtances of an experimental nature; becauſe the reputation of one philoſopher ſtands higher than that of another does, as a conductor of experiments; on this ſuppoſition, the deciſions of the former ought to be preferred to thoſe of the latter, when they happen to diſagree in their concluſions; in fact every man's confidence in the juſtice of an experiment which he has not ſeen, amounts to nothing more than hiſtorical evidence, the credit of the narrative reſting entirely upon the character and abilities of the eye witneſs.

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* Lavoiſier, Mem. Acad. Scienc. 1784.

Perſonal reſpectability is of weight in hiſtory;

but not in argument or reaſoning.

The foregoing obſervations on experimental knowledge have nothing to do with argument; for every ſpecies of true logic, including the mathematics, conſiſts of a train of inductions; all of which are drawn from maxims, admitted by every party, in a well conducted diſpute. On this account, argumentation is not an appeal to a perſon's faith, but to his rational faculties; and an unprejudiced judge will prefer the logic of a ploughman to that of Ariſtotle himſelf, ſhould the ruſtic happen to reaſon better than this maſter of the art. After what has been ſaid on the weight of authority in philoſophical controverſy, it will not be expected that I ſhall pay the leaſt deference to the dogma of any man in the world, unleſs his opinion is ſupported by demonſtration; for this is the only literary authority to which an enquirer after the truth ought to bow. Conſiſtently with this declaration, an attempt has been made to demonſtrate every propoſition that has been advanced on my part, in the preſent diſpute; a diſpute which commenced in the tranſactions of the Royal Society, which paſſed from that publication to the Mancheſter Memoirs, and is now prolonged, Sir, in your Journal. But my care in this reſpect, and attachment to proof have not been imitated by my opponent; for nothing appears in the courſe of the controverſy, reſembling a refutation of any one of my concluſions, excepting an objection which was offered in Dr. Young's laſt letter, to

The preſent controverſy not determinable by authority.

to the theory of the grave harmonics. The Doctor obſerves, that two uniſons, the vibrations of which biſect each other, ought to give the octave, according to my principles. To this I take the liberty to reply, that no two perfect uniſons produce a cycle; becauſe the arrangement which is given to their firſt vibrations, will evidently prevail to the end of the experiment: no points of diviſion can therefore be formed for the ſeparation of the contiguous cycles; in other words, no cycles will be produced, conſequently no imaginary ſound can be perceived in the courſe of the Doctor's experiment, becauſe the exiſtence of the grave harmonics depends upon a ſucceſſion of minute cycles, if my idea of their origin be correct. The effect of two imperfect uniſons will perhaps appear upon examination, not leſs unfavourable to the Doctor's objection, than the preceding caſe: conſonances of this deſcription, it is true, always run into cycles; but then they are too long to give riſe to continuous ſounds, being better fitted to produce beats. For inſtance, let two homogeneous cords of equal diameters be ſtretched with equal forces; and ſuppoſe their lengths to be 20 and 20,1 inches, then the longer will make 200 vibrations, while the ſhorter compleats 201. But the cord correſponding to the grave harmonic of this conſonance, vibrates but once in the ſame time; therefore the length of it is to 20,1 inches as 200 to 1; or it is 335 feet. The difficulty of conducting ſuch an experiment muſt be obvious to every one; for whether ſtrings or pipes be uſed, the lower note of this conſonance muſt compleat at leaſt twelve cycles, or 2400 vibrations in a ſecond; otherwiſe a grave harmonic cannot reſult from this combination of imperfect uniſons.

That two uniſons cannot give the octave by biſection, inſiſted becauſe they cannot form a cycle.

Imperfect uniſons produce beats.

Having now replied to the Doctor's objection, apparently in a ſatisfactory manner, I may without the imputation of rudeneſs, requeſt him to conſider in what way the ſeveral properties of theſe ſecondary ſounds are to be reconciled to his ſyſtem, more eſpecially their want of a fixed direction: ſhould he fail in the attempt, he needs not be reminded that every theory is imperfect, which explains a part of a phenomenon, but does not embrace the whole of it. The want of direction appeared to me ſo plain a proof of the nature of the grave harmonics, the firſt time I made the experiment, that I immediately explained the fact on the principles, which are given at the beginning of this volume of your Journal, without once ſuppoſing the theory

How the want of direction, &c. can be reconciled to Dr. Young's ſyſtem?

to be new; before Dr. Young's laſt letter informed me of the circumſtance. Every attempt to account for this ſingular effect of conſonances by pulſes tranſmitted to the ear through the air, will in all probability encounter the obſtacle already mentioned: for the notion of force compriſes the idea of direction, as well as that of power; and if the latter property cannot be uſed independent of the former, every theory founded on this principle muſt be contradicted by obſervation. I do not pretend to form a judgment on the work of M. Lagrange, from Dr. Young's extracts; and perſons reſiding at a diſtance from the metropolis, as I do, ſeldom enjoy acceſs to uncommon books, ſuch as the Miſcellanea Taurinenſia. However, I agree to the propoſition of M. Lagrange, that a particle of air agitated by two ſounds, acquires a motion differing from that, which each of them would give to it ſeparately: at the ſame time I muſt withhold my aſſent to the ſequel, if I underſtand it rightly; viz. that the ſounds coaleſce in conſequence of this motion, until my arguments on this ſubject have been refuted. While this remains unattempted, the prolongation of the preſent controverſy promiſes to be of no advantage to ſcience: I will therefore in future diſregard all remarks and objections which do not attack the fundamental principle of my theory.

JOHN GOUGH.

Middleſhaw near Kendal, Feb. 11, 1803.

III

Obſervations on the two lately diſcovered celeſtial Bodies. By WILLIAM HERSCHEL, *LL. D. F. R. S.*

(Concluded from Page 128.*)*

WITH regard to the fifth, concerning ſatellites, it may not be eaſy to prove a negative; though even that, as far as it can be done, has been ſhewn. But the retention of a ſatellite in its orbit, it is well known, requires a proper maſs of matter in the central body, which it is evident theſe ſtars do not contain.

The ſixth article ſeems to exclude theſe ſtars from the condition of planets. The ſmall comas which they ſhew, give them ſo far the reſemblance of comets, that in this reſpect we ſhould be rather inclined to rank them in that order, did other circumſtances permit us to aſſent to this idea.

In the ſeventh article, they are again unlike planets; for it appears, that their orbits are too near each other to agree with the general harmony that takes place among the reſt; perhaps one of them might be brought in, to fill up a ſeeming vacancy between Mars and Jupiter. There is a certain regularity in the arrangement of planetary orbits, which has been pointed out by a very intelligent aſtronomer, ſo long ago as the year 1772; but this, by the admiſſion of the two new ſtars into the order of planets, would be completely overturned; whereas, if they are of a different ſpecies, it may ſtill remain eſtabliſhed.

The new ſtars are not planets.

As we have now ſufficiently ſhewn that our new ſtars cannot be called planets, we proceed to compare them alſo with the other propoſed ſpecies of celeſtial bodies, namely, comets. The criteria by which we have hitherto diſtinguiſhed theſe from planets, may be enumerated as follows.

Criterions of comets.

1. They are celeſtial bodies, generally of a very ſmall ſize, though how far this may be limited, is yet unknown. — 1. They are ſmall.
2. They move in very excentric ellipſes, or apparently parabolic arches, round the ſun. — 2. Orbits excentric.
3. The planes of their motion admit of the greateſt variety in their ſituation. — 3. Planes various.
4. The direction of their motion alſo is totally undetermined. — 4. Direction various.
5. They have atmoſpheres of very great extent, which ſhew themſelves in various forms of tails, coma, hazineſs, &c. — 5. Great atmoſpheres.

Compariſon of the new ſtars in theſe reſpects.

On caſting our eye over theſe diſtinguiſhing marks, it appears, that in the firſt point, relating to ſize, our new ſtars agree ſufficiently well; for the magnitude of comets is not only ſmall, but very unlimited. Mr. Pigott's comet, for inſtance, of the year 1781, ſeemed to have ſome kind of nucleus; though its magnitude was ſo ill defined, that I probably overrated it much, when, November 22, I gueſſed it might amount to 3 or 4″ in diameter. But, even this, conſidering its nearneſs to the earth, proves it to have been very ſmall.

That of the year 1783, alſo diſcovered by Mr. Pigott, I ſaw to more advantage, in the meridian, with a 20-feet reflector. It had a ſmall nucleus, which, November 29, was coarſely eſtimated to be of perhaps 3″ diameter. In all my other pretty numerous obſervations of comets, it is expreſsly remarked, that they had none that could be ſeen. Beſides, what I have called a nucleus, would ſtill be far from what I now ſhould have meaſured as a diſk; to conſtitute which, a more determined outline is required.

In the ſecond article, their motions differ much from that of comets; for, ſo far as we have at preſent an account of the orbits of theſe new ſtars, they move in ellipſes which are not very excentric.

Nor are the ſituations of the planes of their orbits ſo much unlike thoſe of the planets, that we ſhould think it neceſſary to bring them under the third article of comets, which leaves them quite unlimited.

In the fourth article, relating to the direction of their motion, theſe ſtars agree with planets, rather than with comets.

The fifth article, which refers to the atmoſphere of comets, ſeems to point out theſe ſtars as belonging to that claſs; it will, however, on a more particular examination, appear that the difference is far too conſiderable to allow us to call them comets.

Account of the ſize of tails of comets.

The following account of the ſize of the comas of the ſmalleſt comets I have obſerved, will ſhew that they are beyond compariſon larger than thoſe of our new ſtars.

Nov. 22, 1781. Mr. Pigott's comet had a coma of 5 or 6′ in diameter.

Nov. 29, 1783. Another of Mr. Pigott's comets had a coma of 8′ in diameter.

Dec. 22, 1788. My ſiſter's comet had a coma of 5 or 6′ in diameter.

Jan. 9, 1790. Another of her comets was ſurrounded by hazineſs of 5 or 6′ in diameter.

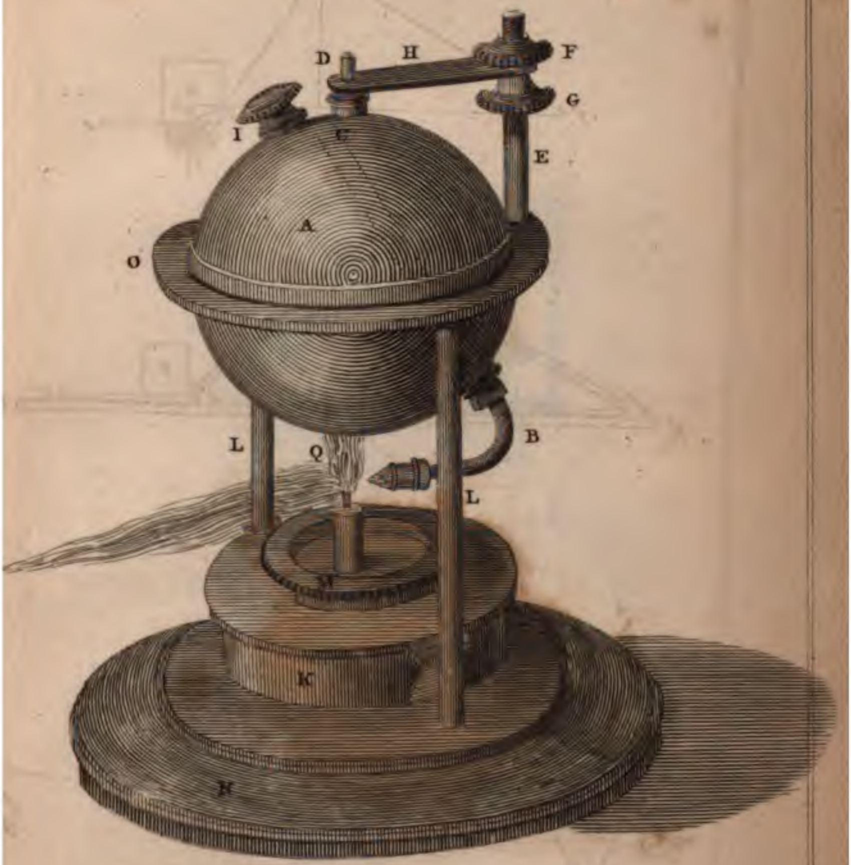
Jan. 18, 1790. Mr. Mechain's comet had a coma of 5 or 6′ in diameter.

Nov. 7, 1795. My ſiſter's comet had a coma of 5 or 6′ in diameter.

Sept. 8, 1799. Mr. Stephen Lee's comet had a coma of not leſs than 10′ in diameter, and alſo a ſmall tail of 15′ in length.

From theſe obſervations, which give us the dimenſions of the comas of the ſmalleſt comets that have been obſerved with good inſtruments, we conclude, that the comas of theſe new ſtars, which at moſt amount only to a few times the diameter of

Blow Pipe, by Alcohol. By Mr. Hooke.



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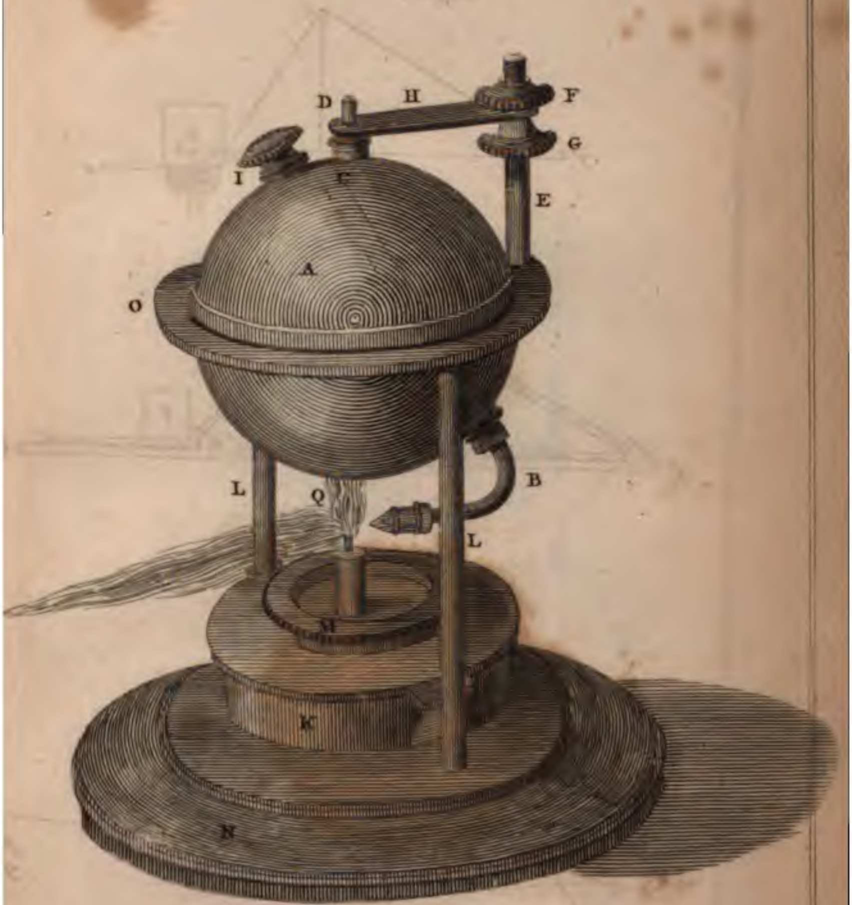


Mutlow, Sc. Russell Ct.

Dr. Young's Harmonic Sliders

Fig. 1.

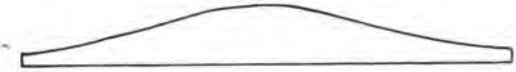


Fig. 2.

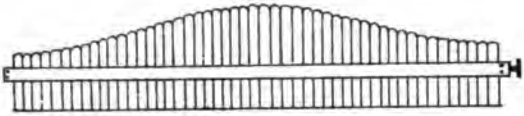


Fig. 3.

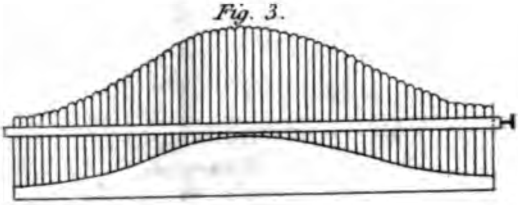


Fig. 4.

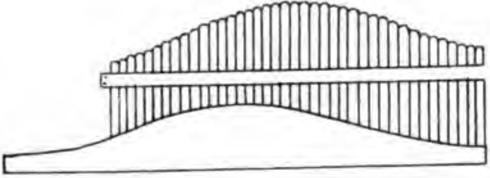


Fig. 5.

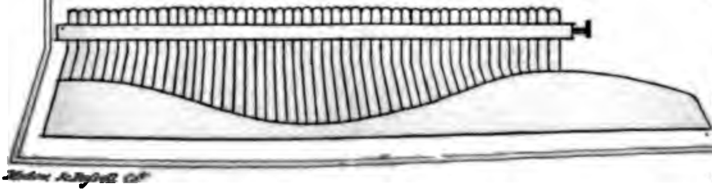
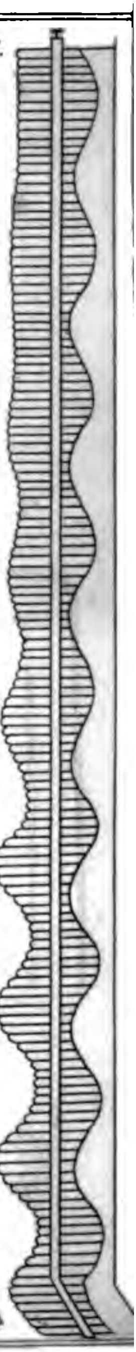


Fig. 6.



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Steam-cocks, Joints & Tubes, W. N.

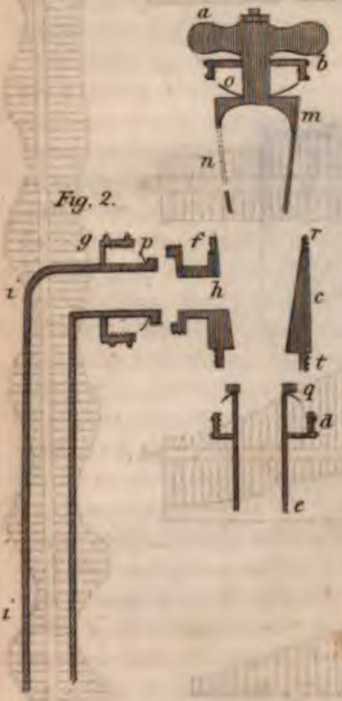


Fig. 2.

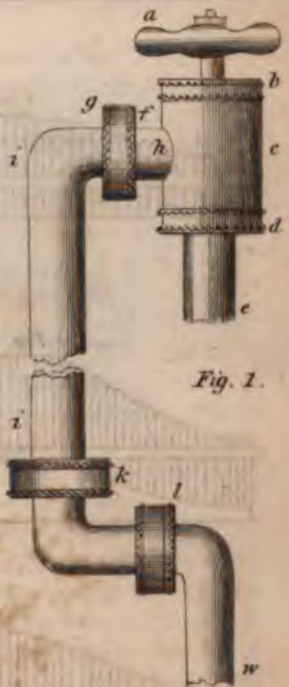
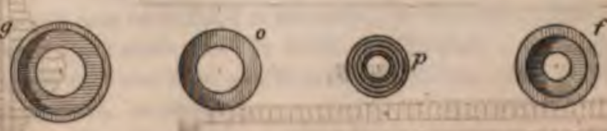


Fig. 1.



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Dr. Young's Harmonic Sliders.

Fig. 1.

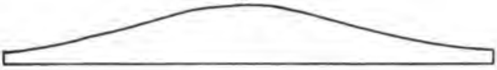


Fig. 2.

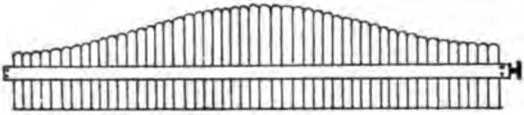
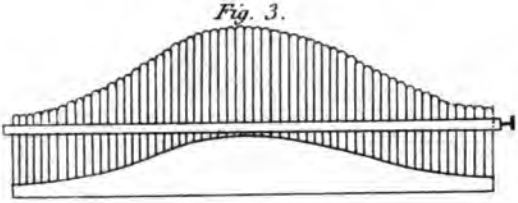


Fig. 3.



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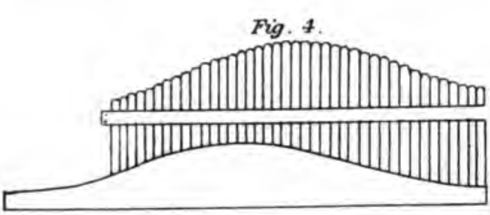


Fig. 5.

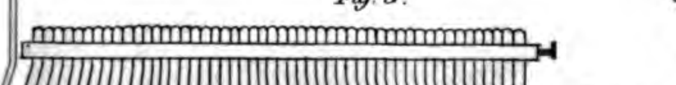
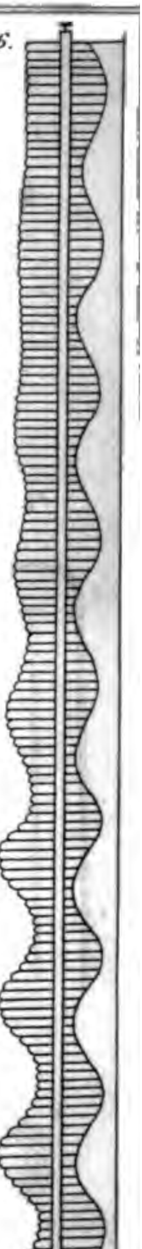


Fig. 6.



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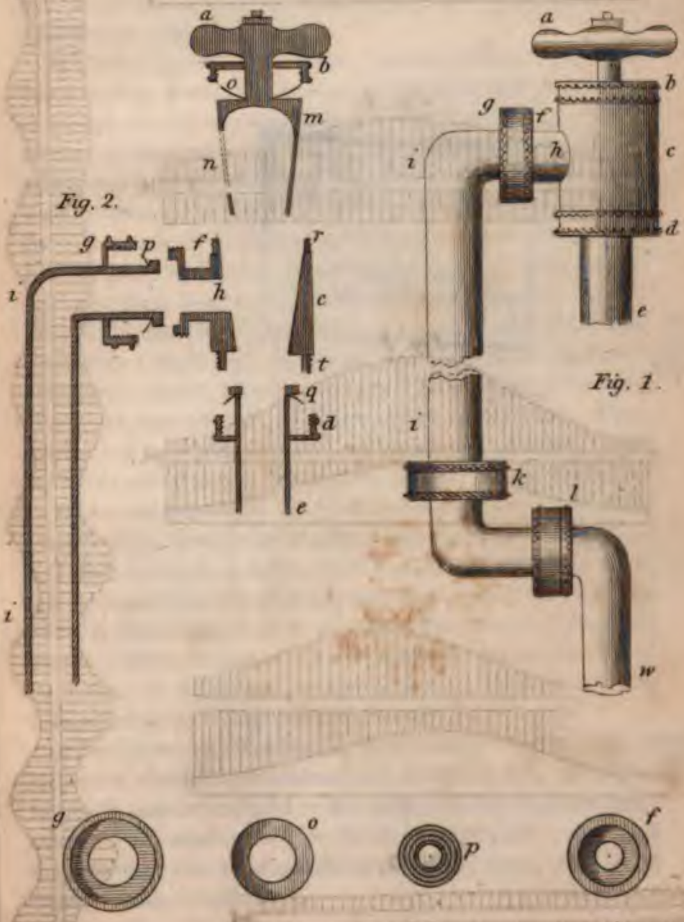
Steam-cocks, Joints & Tubes, Wm. N.



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of the bodies to which they belong, bear no reſemblance to the comas of comets, which, even when ſmalleſt, exceed theirs above a hundred times. Not to mention the extenſive atmoſpheres, and aſtoniſhing length of the tails, of ſome comets that have been obſerved, to which theſe new ſtars have nothing in the leaſt ſimilar.

Theſe new ſtars cannot with propriety of language be called planets or comets.

Since, therefore, neither the appellation of planets, nor that of comets, can with any propriety of language be given to theſe two ſtars, we ought to diſtinguiſh them by a new name, denoting a ſpecies of celeſtial bodies hitherto unknown to us, but which the intereſting diſcoveries of Mr. Piazzi and Dr. Olbers have brought to light.

The author propoſes to call them *Aſteroids*.

With this intention, therefore, I have endeavoured to find out a leading feature in the character of theſe new ſtars; and, as planets are diſtinguiſhed from the fixed ſtars by their viſible change of ſituation in the zodiac, and comets by their remarkable comas, ſo the quality in which theſe objects differ conſiderably from the two former ſpecies, is that they reſemble ſmall ſtars ſo much as hardly to be diſtinguiſhed from them, even by very good teleſcopes. It is owing to this very circumſtance, that they have been ſo long concealed from our view. From this, their aſteroidical appearance, if I may uſe that expreſſion, therefore, I ſhall take my name, and call them *Aſteroids*: reſerving to myſelf, however, the liberty of changing that name, if another, more expreſſive of their nature, ſhould occur. Theſe bodies will hold a middle rank, between the two ſpecies that were known before; ſo that planets, aſteroids, and comets, will in future comprehend all the primary celeſtial bodies that either remain with, or only occaſionally viſit, our ſolar ſyſtem.

I ſhall now give a definition of our new aſtronomical term, which ought to be conſiderably extenſive, that it may not only take in the aſteroid Ceres, as well as the aſteroid Pallas, but that any other aſteroid which may hereafter be diſcovered, let its motion or ſituation be whatever it may, ſhall alſo be fully delineated by it. This will ſtand as follows.

Definition of aſteroids.

Aſteroids are celeſtial bodies, which move in orbits either of little or of conſiderable excentricity round the ſun, the plane of which may be inclined to the ecliptic in any angle whatſoever. Their motion may be direct, or retrograde; and they may or may not have conſiderable atmoſpheres, very ſmall comas, diſks, or nuclei.

Others will ſoon be found out;

As I have given a definition which is ſufficiently extenſive to take in future diſcoveries, it may be proper to ſtate the reaſons we have for expecting that additional aſteroids may probably be ſoon found out. From the appearance of Ceres and Pallas it is evident, that the diſcovery of aſteroids requires a particular method of examining the heavens, which hitherto aſtronomers have not been in the habit of uſing. I have already made five reviews of the zodiac, without detecting any of theſe concealed objects. Had they been leſs reſembling the ſmall ſtars of the heavens, I muſt have diſcovered them. But the method which will now be put in practice, will completely obviate all difficulty ariſing from the aſteroidical appearance of theſe objects; as their motion, and not their appearance, will in future be the mark to which the attention of obſervers will be directed.

by obſerving the relative motion as well as the appearance of ſtars.

Society for that purpoſe.

A laudable zeal has induced a ſet of gentlemen on the Continent, to form an aſſociation for the examination of the zodiac. I hope they will extend their attention, by degrees, to every part of the heavens; and that the honourable diſtinction which is juſtly due to the ſucceſsful inveſtigators of nature, will induce many to join in the meritorious purſuit. As the new method of obſerving the zodiac has already produced ſuch intereſting diſcoveries, we have reaſon to believe that a number of aſteroids may remain concealed; for, how improbable it would be, that if there were but two, they ſhould have been ſo near together as almoſt to force themſelves to our notice. But a more extended conſideration adds to the probability that many of them may ſoon be diſcovered. It is well known that the comas and tails of comets gradually increaſe in their approach to the ſun, and contract again when they retire into the diſtant regions of ſpace. Hence we have reaſon to expect, that when comets have been a conſiderable time in retirement, their comas may ſubſide, if not intirely, at leaſt ſufficiently to make them aſſume the reſemblance of ſtars; that is, to become aſteroids, in which ſtate we have a good chance to detect them. It is true that comets ſoon grow ſo faint, in retiring from their perihelia, that we loſe ſight of them; but, if their comas, which are generally of great extent, ſhould be compreſſed into a ſpace ſo ſmall as the diameters of our two aſteroids, we can hardly entertain a doubt but that they would again become viſible with good teleſcopes. Now, ſhould we ſee a comet in its aphelion, under the conditions here pointed out, and that there are many which may be in ſuch ſituations, we have the greateſt

Comets may become aſteroids;

and be obſerved in their aphelions.

greateſt inducements to believe, it would be a favourable circumſtance to lead us to a more perfect knowledge of the nature of comets and their orbits; for inſtance, the comet of the year 1770, which Mr. Lexell has ſhewn to have moved in an elliptical orbit, ſuch as would make the time of its periodical return only about 5½ years: if this ſhould ſtill remain in our ſyſtem, which is however doubtful, we ought to look for it under the form of an aſteroid.

If theſe conſiderations ſhould be admitted, it might be objected, that aſteroids were only comets in diſguiſe; but, if we were to allow that comets, aſteroids, and even planets, might poſſibly be the ſame ſort of celeſtial bodies under different circumſtances, the neceſſary diſtinction ariſing from ſuch difference, would fully authoriſe us to call them by different names.

It is to be hoped that time will ſoon throw a greater light upon this ſubject; for which reaſon, it would be premature to add any other remarks, though many extenſive views relating to the ſolar ſyſtem might certainly be hinted at.

Additional Obſervations relating to the Appearances of the Aſteroids Ceres and Pallas.

Additional obſervations.

The comas do not much exceed thoſe of the fixed ſtars.

May 4, 12h 40′. 10-feet reflector; power 516½. I compared Ceres with two fixed ſtars, which, in the finder, appeared to be of very nearly the ſame magnitude with the aſteroid, and found that its coma exceeds their aberration but in a very ſmall degree.

12h 50′. 20-feet reflector; power 477. I viewed Ceres, in order to compare its appearance with regard to hazineſs, aberration, atmoſphere, or coma, whatever we may call it, to the ſame phenomena of the fixed ſtars; and found that the coma of the aſteroid did not much exceed that of the ſtars.

I alſo found, that even the fixed ſtars differ conſiderably in this reſpect among themſelves. The ſmaller they are, the larger in proportion will the attendant hazineſs ſhew itſelf. A ſtar that is ſcarcely perceptible, becomes a ſmall nebuloſity.

10-feet reflector. 13h 10′. I compared the appearance of Pallas with two equal fixed ſtars; and found that the coma of this aſteroid but very little exceeds the aberration of the ſtars.

14h 5′, 20-feet reflector. I viewed Pallas; and, with a magnifying power of 477, its diſk was viſible. The coma of this aſteroid is a little ſtronger than that which fixed ſtars of the ſame ſize generally have.

IV.

On the oblique Refraction of Iceland Cryſtal. *By* WILLIAM HYDE WOLLASTON, *M. D. F. R. S.* *From the Phil. Tranſ. for* 1802.

IN the preceding communication *, I have inſerted two different meaſures of refractive powers, diſtinctly obſervable in the Iceland cryſtal, as well as an eſtimate of its diſperſive power; but have reſerved for a ſeparate treatiſe, ſome renarks which the ſame mode of inveſtigation has enabled me to make on its oblique refraction.

Iceland cryſtal well examined by Huygens.

The optical properties of this body have been ſo amply deſcribed by Huygens in his *Traité de la Lumière*, that it could anſwer little purpoſe to attempt to make any addition to thoſe which he has enumerated. But, as the law to which he has reduced the oblique refractions occaſioned by it, could not be verified by former methods of meaſurement, without conſiderable difficulty, it may be worth while to offer a new and eaſy proof of the juſtneſs of his concluſions. For, ſince the theory by which he was guided in his inquiries. affords (as has lately been ſhown by Dr. Young †) a ſimple explanation of ſeveral phenomena not yet accounted for by any other hypotheſis, it muſt be admitted that it is entitled to a higher degree of conſideration than it has in general received.

His theory,

that light is propagated by vibrations of a medium; undulating ſpherically in moſt caſes; but in an oblate ſpheroid, in Iceland cryſtal:

According to that hypotheſis, light proceeding from any luminous centre, is propagated by vibrations of a medium highly elaſtic, that pervades all ſpace. In ordinary caſes, the incipient undulations are of a ſpherical form; but, in the Iceland cryſtal, light appeared to Huygens to proceed as if the undulations were portions of an oblate ſpheroid, of which the axis is parallel to the ſhort diagonal of an equilateral piece of the cryſtal and its centre the point of incidence of the ray.

Whence the law of this refraction: viz. that the fine of incidence has a conſtant rario to the ordinate of refraction in the ſpheroid.

From this ſpheroidical form of the undulations, he deduces the obliquity of refraction; and lays down a law, obſervable in all refractions, at any ſurface of the ſpar, whether natural or artificial, which bears the cloſeſt analogy to that which obtains univerſally at other refracting ſurfaces; for as, in other

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* See our Journal IV. 89.

† Bakerian Lecture. Phil. Trans. for 1801.

eaſes,

caſes, the ratio is given between the ſine of incidence and ſine of refraction, (or ordinate of the *ſpherical* undulation propagated,) ſo, in the Iceland cryſtal, the ratio between the ſine of incidence and ordinate of refraction (in any one ſection of the *ſperoidical* undulation) is a given ratio.

Illuſtration by diagram.

If ABD (Fig. 1, Plate VIII.) be any ſurface of the ſpar, let FHOK be a ſection of the ſpheroid through its centre C, and RC any ray of light falling on that ſurface; draw FO a diameter of the ſpheroid, in the plane of incidence RVO, and CT, its ſemiconjugate diameter, in the plane of refraction FTO. Then, if CI be the refracted ray, VR, the ſine of incidence, ſhall be to EI, the ordinate of refraction parallel to FC, in the conſtant ratio of a given line N to the ſemidiameter FC.

But the refraction will differ, in different planes of incidence, as the diameter of the ellipſe, &c.

In any other plane of incidence, the ratio of ſine to ordinate is alſo conſtant; but it is a different ratio, according to the magnitude of that diameter in which the plane of incidence interſects the ellipſe FHOK.

When the incidence of the ray paſſing from any medium of greater denſity upon the ſurface of this ſpar, is ſuch that the emergent ray becomes parallel to the ſurface, the ordinate of refraction is then a ſemidiameter of the ſpheroid; and, accordingly, the refractive power of this ſpar, when examined by means of a priſm in different directions, ſhould be found to vary as that ſemidiameter which coincides with the plane of incidence and refracting ſurface.

as may be ſeen by the angle of total reflection.

Hypotheſis of Huygens confirmed.

The obſervations that I have made on this ſubſtance, accord throughout with this hypotheſis of Huygens; the meaſures that I have taken correſpond more nearly than could well happen to a falſe theory, and are the more to be depended on, as all my experiments, excepting the laſt, were made prior to my acquaintance with the theory, and their agreement was deduced by ſubſequent computation.

Experiments on the oblique refraction in different planes.

Exp. 1. The oblique refraction of this ſpar is rendered viſible, by cementing a ſurface of it to a priſm of flint-glaſs, with a little balſam of Tolu. When the line of ſight beſects an acute angle of a natural ſurface of the ſpar, the refractive power is ſeen to be leſs than in any other direction, and may be expreſſed by the ſine 1,488, or its reciprocal 0,67204.

Exp. 2. When the plane of incidence is parallel to one of the ſides, the power is 1,518, of which the reciprocal is 0,6587.

Exp. 3. In a direction at right angles with either ſide, it is found ſtill higher, being 1,537, or its reciprocal 0,6506.

Exp. 4. And in the plane biſecting an obtuſe angle, the refractive power of the natural ſurface appears greater, and is expreſſed by the ſine 1,571, or its reciprocal 0,6365.

and ſurfaces.

Exp. 5. When either of the two greateſt ſolid angles of the ſpar contained under three obtuſe angles, is cut off by a poliſhed ſurface making equal angles with each of its ſides, the ſame refractive power 1,488 is found in all directions. By the theory alſo, the ſection of the ſpheroid is in this caſe a circle, and every ſemidiameter (FC) the ſame, ſince the plane is at right angles to the minor axis.

Exp. 6. If a plane ſurface be formed biſecting an obtuſe angle of the ſpar, and applied to a priſm, the ſame minimum of refraction 1,488, is found in a direction that coincides with the preceding plane, and therefore with the major axis of the generating ellipſe; but, as the direction is varied, it increaſes ſo rapidly as ſoon to exceed the power of glaſs, and to be no longer aſcertainable by the angle of incipient reflection.

Regular refraction.

Exp. 7. The regular refraction of this ſpar is alſo too great for examination by means of any priſm, for want of a medium of union of ſufficient denſity; but, by trial in the uſual method, it meaſured, on an average of ſeveral experiments, 1,657, or its reciprocal 0,6035.

Spheroid which regulates the refractions;

By aſſuming, as Huygens has done, the equality of this power with the maximum of the oblique refraction, we have ſufficient data for conſtruction of the ſpheroid by which the refractions are regulated; for we have 0,67204 (*Exp.* 1.) as major axis of the generating ellipſe, and 0,6035 (*Exp.* 7) will be the minor axis, parallel in poſition to the ſhort axis of the ſpar.

The angle of inclination of this axis to the ſurfaces of the ſpar, if ſuppoſed to be equilateral, may be computed by ſpherical trigonometry, from any other angle that has been aſcertained by meaſurement.

The meaſures that I have taken are not exactly thoſe of Huygens; but I nevertheleſs hold them in equal eſtimation, from the conformity which I find they bear to each other, by aſſiſtance of his theory.

Deduction of its dimenſions and poſition.

Exp. 8. I meaſured with care an angle at which two ſurfaces of the ſpar are inclined to each other, and found it to be 105° 5'.

Hence, the greater angle of the ſurfaces themſelves may be computed to be 101° 55′; and the angle which the ſhort axis makes with each plane ſurface is 45° 23′ 25″.

If GSMP (Fig. 2.) be a plane biſecting an obtuſe angle of the ſpar, the ſection of the ſpheroid in that plane paſſes through the axis CS, and therefore is the generating ellipſe. By calculating from the known dimenſions of its major axis CP 0,67204, its minor axis CS 0,6035, and the angle GCS=45° 23′ 25″, CG will be found [*] to be 0,6365, of which the reciprocal is 1,5736, differing but little from 1,571, as it appeared by meaſurement. (*Exp.* 4.)

Again, if ABDE (Fig. 4.) be one of the natural ſurfaces, and PG*p* the ellipſe formed by that ſection of the ſpheroid, PC being as before 0,67204, and CG 0,6365, the reciprocal of 1,571 found by meaſurement, (*Exp.* 4) then the ſemidiameter CT, parallel to the ſide AE, which makes an angle TCP 39° 2½′, will be found to be 0,6573, inſtead of 0,6587, and its reciprocal 1,5215, inſtead of 1,518. (*Exp.* 2.)

The ſemidiameter alſo, in the direction of CL, perpendicular to the ſide, at an angle LCP 50° 57½′, is found by calculation 0,650, and its reciprocal 1,539, inſtead of 0,6506 and 1,537. (*Exp.* 3.)

Courſe of a perpendicular ray after this refraction.

From the foregoing data, the courſe of a ray perpendicular to the ſurface of the ſpar may likewiſe be computed; for, ſince the ſine of incidence is then nothing, the ordinate of refraction muſt be alſo nothing, and the ray will be refracted along the ſemiconjugate diameter CM. (Fig. 2.)

By calculation [†], the angle which this conjugate makes with the perpendicular is 6° 7½′. But, by the following meaſurement, it appears to be 6° 16′.

Experiment to ſhew the ſame courſe.

Exp. 9. A piece of ſpar that meaſured 1,145 inch in thickneſs, was laid upon a line, and ſhowed two images that were removed from each other $\frac{126}{1000}$ of an inch. Then, as 1,145 : 0,126 :: radius : tang. of 6° 16′.

Comparative ſtatement of reſults from obſervation and from theory.

The different reſults deduced from theory and from obſervation, will be ſeen at one view in the following ſtatement:

[*] (Fig. 3.) CS : CP :: tang. PCG : tang. PC*p* ſec. PC*p* : ſec. PCG :: CP : CG.

[†] (Fig. 5.) CS : CP :: tang. PCG : tang. *p*CQ or co-tang. PCQ; then CP : CS :: tang. PCQ : tang. PCM; and LCP—PCM=MCL.

In *Exp.* 2d, obſerved 1,518; calculated 1,5215.
3d, —— 1,537 —— 1,539.
4th, —— 1,571 —— 1,5736.
6th, angle obſerved 6° 16′ - - -6° 7½′.

Remarks.

The angle obſerved differs from that obtained by computation, in a greater degree than any of the former meaſures; but, when the difficulty of meaſuring this angle with accuracy is conſidered, and alſo the greater effect of any incorrectneſs in the data from which a ſemiconjugate is computed, I think the reſult of this, as well of the preceding compariſons, muſt be admitted to be highly favourable to the Huygenian theory; and, although the exiſtence of two refractions at the ſame time, in the ſame ſubſtance, be not well accounted for, and ſtill leſs their interchange with each other, when a ray of light is made to paſs through a ſecond piece of ſpar ſituated tranſverſely to the firſt, yet the oblique refraction, when conſidered alone, ſeems nearly as well explained as any other optical phenomenon.

V.

The Theory of Compound Sounds, *By Mr.* JOHN GOUGH *.

Dr. Smith's theory of compound ſounds; that the pulſes do not obſtruct or become confounded with each other.

DR. SMITH, author of the work on Harmonics, takes for granted in his theory of compound ſounds, that the pulſes which proceed at the ſame time from a number of ſounding bodies, do not claſh, or obſtruct one another, in their paſſage through the air. According to this hypotheſis, each ſet, of any number of cotemporary ſets of pulſes, ſtrikes the ear without being confounded with the reſt; in conſequence of which, any number of ſounds may be diſtinctly perceived at the ſame time. On this ſuppoſition, a compound ſound is a ſenſation rendered variable by the irregular manner in which the pulſes of the conſtituent ſounds ſucceed to one another. For, if the intervals of time between two ſucceſſive pulſes of one of the conſtituent ſounds be not equal to the ſame intervals belonging to the ſound or ſounds which accompany it, the ſecondary intervals, or ſmall parts of time ſeparating the pulſes which fall in ſucceſſion on the ear, will vary in magnitude; in the ſame manner that the diſtances between the figures upon the face of a barometer and its nonius vary, none on the ſlider coinciding with thoſe on the fixed plate excepting the higheſt and loweſt. I have choſen this familiar inſtrument to illuſtrate Dr. Smith's method of explaining the phyſical cauſe of compound ſounds, becauſe it affords a viſible example of a cycle of pulſes, according to his notion of the ſubject.

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* Mancheſter Memoirs, v. 653. This treatiſe which was not inſerted at an earlier period in our Journal, on account of the preſſure of other matter, has become of ſtill more intereſt as containing the theory to which the author's late diſcuſſion with Dr. Young is directed.—W. N.

The intervals of the pulſes are not equal, but increaſe and diminiſh periodically, as in a ſcale and nonius.

The ſketch which I have exhibited of Dr. Smith's hypotheſis ſhews, that he allowed that a number of ſimple ſounds might exiſt in concert, and ſtrike the ear in a diſtinct manner, without ſuffering any interruption in their motions from the interference of their pulſes. But a late writer on ſound rejects this axiom in Harmonics as a mathematical inconſiſtency; and ſubſtitutes the following theory of compound ſounds in the room of it. If two muſical ſtrings, differing in their times of vibration, happen to vibrate in concert, they do not occaſion two diſtinct ſounds in the opinion of this gentleman, becauſe the ſtrings agitate the air in conjunction; conſequently the pulſes, which one of them would actually form in an undiſturbed atmoſphere, muſt unavoidably claſh with thoſe which the other ſtring would produce in ſimilar circumſtances. Hence the waves of air belonging to both ſtrings are interrupted in their natural progreſs, and are compelled by their mutual interference to coaleſce, thereby producing a new ſucceſſion of pulſes, conſtituting a ſingle ſound in the place of two. This ſound is of a peculiar kind; for, the pulſes of which it conſiſts, are ſeparated by unequal intervals of time, and diſpoſed in cycles.

Another theory (Dr. Young's), that the pulſes coaleſce; and form a peculiar ſound, having its pulſes unequal and diſpoſed in cycles.

The merit of the preceding theory, when compared with Dr. Smith's hypotheſis, muſt be aſcertained by contraſting it with a variety of facts, which are furniſhed by the phænomena of compound ſounds, and make a part of every man's experience. For, if it be found upon examination to be repugnant to theſe facts, it will prove inconſiſtent with nature, and cannot fail of diſappointing the inventor's expectations.

Examination of the latter theory.

Were it poſſible for a number of ſounds to coaleſce, and form but one, the compound would acquire ſenſible properties peculiar

If ſounds could coaleſce they would form a new compound;

to itſelf, and at the ſame time loſe the diſtinguiſhing characters of its elements, ſome of which are incompatible with the qualities of an individual. On this ſuppoſition, the preſence of the conſtituent ſounds could not be detected by the ear in this newly created being: on the contrary, an experimental proceſs would be required to analyſe every compound ſound the firſt time it attracted a man's attention, for the ſame reaſon that a chemiſt finds it neceſſary to analyſe a ſubſtance with which he is unacquainted. The abſtract term *coaleſcence* is uſed, in a phyſical ſenſe, to ſignify any intimate union of bodies or the powers of bodies; and the introduction of the term into language proves the exiſtence of the principle in nature, or more properly in the human mind. For, when a number of agents act in conjunction upon one of the ſenſes, we have two ways of conceiving their mode of operation. If the ſenſible effects of each agent be diſtinctly perceived, we attribute a ſeparate action to every member of the aſſemblage, and call the aggregate a mixture: this is the concluſion of a perſon who taſtes an infuſion of pepper in vinegar. On the other hand, when we know that certain agents are preſent without being able to recognize their diſtinguiſhing powers, in the room of which we find qualities of a different deſcription, we pronounce the aggregate to be in a ſtate of coaleſcence. This is the ſituation of the chemiſt, who taſtes common ſalt, but cannot perceive the preſence of ſoda and the muriatic acid. It is my buſineſs then to prove compound ſounds to be *mixtures*, not aggregates by coaleſcence. This I ſhall endeavour to do, by ſhewing that they have properties which belong not to individuals, ſuch as a number of tones, a variety of directions, and ſeveral ſets of pulſes.

in which the ear could not diſtinguiſh the component parts.

Chemical compariſon.

Compound ſounds are mixtures; not combinations.

Firſt, the tones of a flute and violin are as diſtinct to ſenſe as any two things can be when they are ſounded ſeparately; and I appeal to common experience to determine, if they are not equally diſtinct when heard in concert. Taking it for granted that the anſwer will be in the affirmative, I pronounce the aggregate to be a mixture of ſounds in one caſe. Secondly, if a violin ſound in front of the hearer, and a flute be heard at the ſame time in an oblique ſituation, the perſon thus circumſtanced is able to determine the relative poſitions of the two inſtruments, which ſhews the aggregate to have two contemporary directions. It is therefore a mixture of ſounds, not a ſingle

Inſtances. The flute and violin perfectly diſtinguiſhable in concert,

the courſes of the two ſounds are diſtinguiſhed when they differ.

a ſingle ſound. Thirdly, I have found by making the experiment, that any number of muſical ſtrings may be made to vibrate by a compound ſound acting upon them, provided this compound be occaſioned by an equal number of ſtrings with the former, having one in the latter ſet in uniſon with each one in the preceding ſet. This is an experimental proof that there are as many ſets of pulſes in an aggregate of ſounds as that aggregate contains elements, becauſe no ſtring whatever is in uniſon with a concord or diſcord. Laſtly, if it were poſſible for ſounds to coaleſce, men would never hear any thing more than one noiſe at one time: The general hum would have varied perpetually from the extinction of exiſting ſounds, and the intruſion of freſh ones; but the human mind would have had no conception of two cotemporary ſounds; becauſe the ear being in that caſe incapable of conveying the complex ſenſation, the idea of ſuch an exiſtence would have tranſgreſſed the ſphere of human knowledge. The preceding arguments are drawn, for the moſt part, from common experience; and they ſhew, that the free paſſage of cotemporary ſounds through the air may be ſafely admitted as an axiom in harmonies. I ſhall therefore proceed to prove the ſame propoſition to be conſiſtent with the doctrine of forces.

A number of ſtrings will be ſet into vibration by the compound ſound of other ſtrings giving uniſon to each reſpectively.

If ſounds coaleſce we ſhould conſtantly hear one varying noiſe.

The propagation of ſound through the atmoſphere, and the nature of aëreal pulſes are commonly explained in elementary books of natural philoſophy; I ſhall, for this reaſon, enumerate only a few particulars, the recollection of which will be found uſeful.

Mechanical arguments.

Propoſition I. Two contiguous particles of air which are agitated by a vibrating body, either directly or by the intervention of an elaſtic medium, receive two motions from each impulſe; *firſt*, an abſolute motion carries them to a greater diſtance from the ſounding body, and afterwards brings them towards it again, both the progreſs and regreſs being performed in the time of a ſingle vibration: *ſecond*, a relative motion reſulting from the former, compels the two particles to approach and recede alternately, which double motion is alſo accompliſhed in the time of a ſingle vibration.

Prop. 1. Vibration of the particles of air, in ſound.

Propoſition II. Both the abſolute and relative motions are greateſt amongſt thoſe particles which are neareſt the ſounding body, and they diminiſh as the diſtance from that body increaſes; but, in all caſes, the change of its place too ſmall to be

Prop. 2. The particles are not ſenſibly removed.

perceived by the ear, on which account every particle preſerves a fixed poſition in reſpect of this organ and its connections.

Phyſical right line.

For each corpuſcle is confined within the circumference of a phyſical right line, the diameter of which is determined by its own abſolute motion.

Prop. 3. A particle in the interſection of two rows of particles conveying different ſounds, will be urged in thoſe lines by forces varying in their ratio.

Propoſition III. If two ſounding bodies, affording different notes, act in conjunction upon the ſame particle, through the media of two right lines of ſimilar particles connecting them with it, this particle will be urged at the ſame inſtant in the direction of theſe lines, by two forces having an inconſtant ratio.

For, let the particle A be urged, by the acuter ſound, in the line SA, and by the graver, in the line TA; *(vide* Fig. 2, Plate IX.); then the contiguous particle V, placed in SA, will approach to, and recede from A more frequently than W, ſimilarly placed in TA, by Prop. I.: conſequently the force of V upon A will vary in a quicker manner than the force of W upon A; but this variation of ratio is limited in time; becauſe it evidently begins and ends with the cycle of the vibrations of the ſounding bodies.

Prop. 4. The right lined motion of the particle neceſſary to form a ſound by coaleſcence is impoſſible.

Propoſition IV. The coaleſcence of two ſounds is impoſſible on mechanical principles.

For, ſuppoſe the thing poſſible; then the coaleſcence of two ſounds requires, that a particle of air ſhould poſſeſs a motion, compounded of the motions which the two ſounding bodies would impart to it ſeparately; and that this compound motion ſhould act in a given right line, for an aſſignable part of time, otherwiſe it could not excite a ſimilar motion in the elaſtic particles occupying that given right line. Let A be ſuch a particle, and let the conſtruction uſed in the laſt propoſition, be retained; conſequently (Principia, Prop. 23, Lib. 2.) VA and AW are in the ratio of the forces that act at any moment in the right lines TA and SA. Make AK as AW, and draw KL parallel to AW, and make it as AV; alſo join AL; then will the particle A be urged in the direction LA at that inſtant. But the ratio of AK to KL varies perpetually, by Prop. III.; therefore the ſpecies of the triangle AKL is equally inconſtant; conſequently the compound force does not act in a given direction for an aſſignable part of time. Now the production and propagation of motion in a given right line requires force to be combined with time, which combination is wanting in the preſent inſtance; wherefore the coaleſcence of ſounds is impoſſible.

Prop. 5. Diſtinct cotemporary ſounds produce diſtinct ſenſations.

Propoſition V. It may be demonſtrated from mechanical principles, that a number of diſtinct cotemporary ſounds cannot do otherwiſe than produce diſtinct ſenſations.

In order to make the neceſſary diagram as ſimple as poſſible, let the directions of two cotemporary ſets of pulſes be repreſented by the right lines SM and TN, lying in the ſame horizontal plane, and interſecting in the point A; alſo, let BCD be the horizontal ſection of the hearer's head, made by the ſame plane; and ſuppoſe the centre of the axis of hearing to be at O; draw OM, ON perpendicular to SM, TN. Now I have ſhewn in the preceding paper*, that if a ſet of pulſes move in either of the right lines SM, TN, it will excite a ſenſation in that part of the head which is cut off by a vertical plane, paſſing through one of the perpendiculars OM, ON. It alſo appears from the laſt propoſition, that the impulſes of the vibrating bodies, acting in the lines SA, TA, do not compel the particle A to move in any given intermediate direction, as LA. But, according to the ſecond propoſition, the poſition of the particle A, is fixed in reſpect of the planes MO, NO; that is, though the corpuſcle actually changes place, in reſpect of the geometrical point A, it is always found in the interſection of the phyſical right lines SM, TN. Now the two vibrating bodies continue to act in the directions of theſe right lines, conſequently the particle A is conſtantly urged in theſe lines by two forces, which, though variable in magnitude, are combined with time; which circumſtance enables the corpuſcle to tranſmit the impulſes of one body to M, and thoſe of the other to N. What has been demonſtrated of the particle A, may be affirmed of any other particle, which is the interſection of two right lines parallel to SM, TN; in other words, it may be affirmed of two ſets of pulſes; and the ſame demonſtration may be extended to three ſets, &c.

Theſe propoſitions apply to ſounds conducted through ſolids, &c.

Corollary 1. The ſubſtance of this and the preceding propoſition will apply to all elaſtic mediums; hence it happens, that a plate of glaſs, &c. in a ſtate of vibration, will conduct a foreign ſound, whilſt it produces one of its own; for the ſame reaſon, if light be conſidered as a vibrating medium, one particle of the luminous fluid may aſſiſt in tranſmitting two ſenſations.

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* Mancheſter Mem. v. 342, or Philoſ. Journal, II. 460.

The directions will not be perceived unleſs they differ by a certain angle.

Corollary 2. When the inclination of the planes MO, NO, is leſs than a given angle, the ear cannot diſtinguiſh the relative poſitions of the ſounding bodies; it therefore refers them to the ſame place.

The firſt time I peruſed Dr. Smith's Harmonics, Dr. Young's objection occurred to me; but the preceding train of arguments removed the ſcruple, without diſcovering the author's reaſons for treating this article of his work with ſo much brevity. Perhaps the demonſtration, which coſt me an effort of ſtudy, was an intuitive concluſion in his comprehenſive mind. As ſoon as the propoſition was eſtabliſhed, I aſſented to his definition of an interval of ſound, allowing it to be a quantity of a certain kind, terminated by a graver and an acuter ſound. The demonſtration of Prop. V. convinced me, that intervals of this ſort may be ſubdivided by the interpoſition of one or more intermediate ſounds, which conceſſion formed the baſis of my analyſis of the human voice.* Speculative men may differ in opinion about the origin of the ſmall intervals which form the tones of various voices; but they muſt exiſt, whether we aſcribe them to an undulating motion like that of a ſtretched cord, or to the cotemporary vibrations of a ſyſtem of elaſtic bodies. It does not appear, that Dr. Young was acquainted with my paper at the time he compoſed his own; but he found it neceſſary to allow the tone of the larynx to receive various modifications from the vibrations of the adjacent parts. His theory therefore differs from mine in this particular only: he pronounces the voice to be a compound by coaleſcence; I deny the poſſiblility of ſuch a compound, and call it a mixture of imperfect uniſons. This mixture appears to be a ſingle ſound, becauſe it has but one direction; for the proximity of the various parts contributing to the formation of it, diſqualifies the ear, ſo that it cannot perceive their relative poſitions, and compels it to refer them all to one place, by Corollary 2 to propoſition V.

Interval of ſound.

Analyſis of the human voice.

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* Mancheſter Mem. v. 58, or Phil. Journal, Quarto, IV. 46. for an abridged ſtatement.

A certain

Claſs of ſounds produced by the quick ſucceſſion of other ſounds; the firſt giving the tune or pitch, and the latter the tone or character.

A certain claſs of ſounds, which, for the ſake of brevity, were not noticed in my paper on the voice, deſerve a place in the preſent communication. If a finely-toothed file paſs ſlowly over a ſmooth elaſtic ſubſtance, ſuch as a piece of horn, it makes a grating noiſe; but if the velocity of the inſtrument be ſufficiently increaſed, a continued ſound is produced, which becomes more or leſs acute, by giving a quicker or ſlower motion to the file. The grating noiſe is occaſioned by a ſucceſſion of ſhort interrupted ſounds, reſulting from the united vibrations of the file and the body it ſcratches; but the quick ſucceſſion of theſe ſounds, cauſed by an increaſe of velocity, gives riſe to a ſecondary ſound reſembling the harmonical notes, being produced by a like cauſe. Now this ſound becomes a primary object with the ear, in all probability becauſe the pitch of it may be varied; for the firſt ſounds proceeding from the action of the file, evidently ſupply nothing but the tone. Many inſtances of the kind occur in art and nature: the notes of all reed-inſtruments are of this deſcription, and the voice muſt be referred to the ſame claſs, becauſe the larynx reſembles a reed-inſtrument in ſtructure.

VI.

Experiments and Obſervations to determine whether the Quantity of Rain and Dew is equal to the Quantity of Water carried off by the Rivers and raiſed by Evaporation; with an Enquiry into the Origin of Springs.

By Mr. JOHN DALTON[*].

Intereſting ſyſtem by which the globe is ſupplied with water.

IT is ſcarcely poſſible to contemplate without admiration the beautiful ſyſtem of nature by which the ſurface of the earth is continually ſupplied with water, and that unceaſing circulation of a fluid ſo eſſentially neceſſary to the very being of the animal and vegetable kingdoms takes place. Naturaliſts, however, are not unanimous in their opinions whether the rain that falls is ſufficient to ſupply the demands of ſprings and rivers, and to afford the earth beſides ſuch a large portion for evaporation as it is well known is raiſed daily. To aſcertain this point is an object of importance to the ſcience of agriculture, and to every concern in which the procuration and

Whether the rain be equal to all the demands of rivers, &c.

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[*] Mancheſter Memoirs, V. 346.

management of water makes a part, whether for domeſtic purpoſes or for the arts and manufactures.

For the ſake of perſpicuity I have diſtributed the ſubject under four heads:

Diviſion of the ſubject.

1. Of the quantity of rain and dew.
2. Of the quantity of water that flows into the ſea.
3. Of the quantity of water raiſed by evaporation.
4. of the origin of ſprings.

SECTION I.

An Eſtimate of the Quantity of Rain and Dew that falls in England and Wales in a Year.

Annual quantity of rain and dew in England and Wales. Inland counties have leſs rain than coaſts; and mountainous diſtricts have moſt.

Rain-gages have been fixed of late years in almoſt every part of the kingdom; by means of them we are enabled to determine, with conſiderable exactneſs, the depth of water that the rain yields in any given place. Inland counties have leſs rain than maritime ones, eſpecially thoſe which border on the weſtern ſeas. But a ſtill greater difference ſeems to take place between a mountainous country and a champaigne, or flat country: in the former there often falls double or triple the quantity of rain in a year, that there does in the latter, and never leſs than an equal quantity. It may be obſerved, that ſeveral years account of the rain at any place is required before a medium yearly quantity can be obtained with ſufficient accuracy. The following is perhaps the largeſt collection of accounts of rain fallen in different places in England that has hitherto appeared: They are moſtly taken from the Tranſactions of the Royal and other Societies.

Quantities tabulated.

Counties (maritime)	*Places.*	Mean annual depth in inches.
CUMBERLAND	Keſwick, 7 years	67. 5
	Carliſle, 1 year	20. 2
WESTMORLAND	Kendal, 11 years	59. 8
	Fell-foot, 3 years	55. 7
	Waith Sutton, 5 years	46
LANCASHIRE	Lancaſter, 10 years	45
	Liverpool, 18 years	34. 4
	Mancheſter, 9 years	33
	Townley	41
	Crawſhawbooth, near Haſlingden, 2 years	60

GLOUCESTERSHIRE

Counties (maritime)	*Places.*	Mean annual depth in inches.
GLOUCESTERSHIRE	Briſtol, 3 years	29. 2
SOMERSETSHIRE	Bridgewater, 3 years	29. 3
CORNWALL	Ludguan, near Mount's Bay, 5 years	41
	Another place, 1 year	29. 9
DEVONSHIRE	Plymouth, 2 years	46. 5
HAMPSHIRE	Selbourne, 9 years	37. 2
	Fyfield, 7 years	25. 9
KENT	Dover, 5 years	37. 5
ESSEX	Upminſter,	19. 5
NORFOLK	Norwich, 13 years	25. 5
YORKSHIRE	Barrowby, near Leeds, 6 years	27. 5
	Garſdale, near Sedbergh, 3 y.	52. 3
NORTHUMBERLAND	Widdrington, 1 year	21. 2

Counties (inland)	*Places.*	*Means.*
MIDDLESEX	London, 7 years	23
SURREY	South Lambeth, 9 years	22. 7
HERTFORDSHIRE	Near Ware, 5 years	25
HUNTINGDONSHIRE	Kimbolton, 7 years	25
DERBYSHIRE	Chatſworth, 15 years	27. 8
RUTLANDSHIRE	Lyndon, 21 years	24. 3
NORTHAMPTONSHIRE	Near Oundle, 14 years	23
	General Mean	35. 2

Mean quantity of rain.

This general mean of 35. 2 inches is, I apprehend, a little above the medium for England and Wales, as the greater number of places are thoſe where much rain falls. If we take a mean for each of the above-mentioned counties (where more than one place in a county is given) and then a general mean from the counties, the reſult is a reduced mean of 31. 3. Even then it may be objected that the greater part of the counties are maritime; but it muſt be obſerved, that there is no account of rain in Wales; and we may ſafely conclude, that the rain in Wales would exceed the laſt-mentioned mean as much as the inland counties of England, not in the above liſt, would fall ſhort; becauſe Wales is both a mountainous country, and expoſed to the ſea.

Corrected is 31 inches for England and Wales.

We will therefore conclude, that the mean annual depth of rain in England and Wales, deduced from theſe 20 counties, is 31 inches: A quantity which ſubſequent obſervations, I am confident, will not diminiſh, and probably not increaſe much *.

Quantity of dew. It is water depoſited by the cold of night.

Eſtimate by Dr. Hales.

Annual quantity of dew taken at five inches.

It remains to eſtimate the quantity of dew that falls in a year. Some have doubted whether dew is derived from the air or the earth; but a proper attention to the phenomena will ſatisfy us, that it is a depoſition of water, evaporated during the heat of the day. With reſpect to the quantity that falls in a year, we are much at a loſs, as no daily obſervations have been made for a ſeries of time that I know of: indeed, it would be difficult to preſcribe a mode of obſervation. Dr. Hales † relates ſome experiments made to determine the quantity of dew that falls upon moiſt earth, from which he eſtimates the annual dew at 3. 28 inches. But it is probable that the dew which is depoſited on graſs is much more copious than what falls on moiſt earth, becauſe graſs expoſes much more ſurface in a given acre of ground. If we take the dew at five inches annually, it will probably not be much over-rated: ſuppoſing it ſhould be over-rated, the exceſs may ſtand againſt the rain that is loſt by evaporation from the ſurface of the rain-gage each time it rains ‡. Wherefore, upon the whole, we ſhall

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* The editors of the Encyclopedia, under the article Weather, from 16 places of obſervation, make the annual mean for Great Britain 32. 53 inches; and M. Cotte, in the Journal de Phyſique for 1791, gives a mean derived from 147 places in different parts of the world equal to 34. 7 inches.

† Veg. Statics, Vol. I. page 52.

‡ Since writing the above paragraph on dew, I have had occaſion to make ſeveral experiments on the ſubject of aqueous vapour, as it exiſts in the atmoſphere, the reſult of which will, I am perſuaded, materially illuſtrate this important queſtion in phyſics.—At preſent I ſhall only obſerve, that the following concluſions ſeem deducible from the experiments above referred to.

Generalities concerning aqueous vapor.

1. That aqueous vapour is an elaſtic fluid *ſui generis*, diffuſible in the atmoſphere, but forming no chemical combination with it.

2. That temperature alone limits the *maximum* of vapour in the atmoſphere.

3. That

ſhall have 36 inches of water at a medium annually on the ſurface of the earth in England and Wales, reckoning 31 for rain and five for dew.

Annual quantity of rain and dew 36 inches.

According to Guthrie, the area of England and Wales is 46.450 ſquare miles. This reduced to ſquare feet, gives 1.378.586.880.000: which, multiplied by three feet the annual depth of rain and dew, gives 4.135.760.690.000 cubic feet of water = 153.176.320,000 cubic yards, or 28 cubic miles = 115 thouſand millions of tons in weight, nearly. We muſt now conſider how this enormous quantity of water is diſpoſed of.

Computed quantity of water that falls in a year 28 cubic miles, or 115 thouſand million of tons.

There are two principal ways by which the water derived from rain is carried off again: one part of it runs off immediately into rivulets, or ſinks into the earth a ſmall way, breaks out again in lower ground in the form of ſprings, thence makes its way to ſome river, by which it is conveyed into the ſea—another part is raiſed into the atmoſphere by evaporation. We take no notice here of the decompoſition of water by vegetables; becauſe it is preſumed that in the courſe of nature the principles are combined and water formed again.

It is carried off by rivers, by ſprings, and by evaporation.

3. That there exiſts at all times, and in all places, a quantity of aqueous vapour in the atmoſphere, variable according to circumſtances.

4. That whatever quantity of aqueous vapour may exiſt in the atmoſphere at any time, a certain temperature may be found, below which a portion of that vapour would unavoidably fall or be depoſited in the form of rain or dew, but above which no ſuch diminution could take place, chemical agency apart. This point may be called the *extreme temperature* of vapour of that denſity.

5. And that whenever any body colder than the extreme temperature of the exiſting vapour is ſituated in the atmoſphere, dew is depoſited upon it, the quantity of which varies as the ſurface of the body and the degree of cold below the extreme temperature.

N. B. The *extreme temperature* of vapour in the atmoſphere varies all the way from the *actual* temperature of the atmoſphere to 10, 15, 20 or more degrees below it.—The point may generally be found in the hotteſt months by pouring cold ſpring water into a dry and clean glaſs, and marking what degree of cold is ſufficient to produce a dew on the outſide of the glaſs; at other times frigorific ſaline ſolutions may be uſed.

SECTION 2.

An Eſtimate of the Quantity of Water that flows into the Sea from England and Wales in a Year.

Method of determining how much water flows through a river.

To calculate the quantity of water that flows down any one river into the ſea in a given time, ſeems at firſt view a queſtion of great difficulty. The neceſſary data, however, may be obtained with conſiderable exactneſs, by proper obſervations, and then it becomes an eaſy caſe of menſuration. Dr. Hutton, in his Philos. and Mathemat. Dictionary, article River, propoſes a very good method to determine by experiment the velocity of a river:—A cylindrical piece of light wood, its length ſomewhat leſs than the depth of the waters, is to be taken, and a few ſmall weights attached to one end in order to make it ſwim upright. To the other end a ſmall rod is fixed in the centre in direction of the axis. This being ſuffered to float down the ſtream will move with the velocity of the water; and if the rod be obſerved to incline towards the river upward or downward, it ſhews the current to be more rapid at the bottom or ſurface reſpectively.

This experiment being made in the middle and near the ſides of a river, a medium velocity may be obtained. Then the medium, breadth, depth, and ſpace run over in a certain time being multiplied together, will give the quantity of water that flows down in that time.

Eſtimate of water delivered by the Thames.

Dr. Halley, in order to eſtimate the quantity of water that flows into the Mediterranean ſea by means of rivers, makes a compariſon of the great rivers of Italy, &c. with that of the Thames. (Philos. Tranſact. Abridg. Vol. 2. Page 110). He aſſumes the breadth of the Thames at Kingſton Bridge to be 100 yards, its depth three yards, and velocity two miles per hour. He profeſſedly over-rates the dimenſions, in order to allow more than a ſufficiency for the ſtreams received below Kingſton. This aſſumption gives the area of a tranſverſe ſection of the river = 300 ſquare yards, and the quantity of water flowing down = 20.300 000 tons in a day. This muſt be over-rated by at leaſt, I think, one third:—If the breadth be aſſumed 100 yards, the depth three, and velocity two miles per hour, it will then give $\frac{2}{3}$ of the reſult above mentioned; or it will amount to the ſame thing if we take $\frac{1}{8}$ part from all the three data aſſumed by Dr. Halley, the reſult being $\frac{2}{3}$ of that above, amounting in the year to 166.624.128.000 cubic feet, which is a little more than $\frac{1}{25}$ part of all the rain and dew in England and Wales in a year, as above deduced.

View of all the rivers of England and Wales,

By an inſpection of the annexed map of the rivers of this country, as well as by a fair calculation, it appears, that the water of the Thames is drawn from an extent of country of about 600 ſquare miles, or $\frac{1}{8}$ of the area of the whole, nearly. The Severn, including the Wye, ſpreads over an equal or greater extent of country: and that collection of rivers which conſtitutes the Humber is ſuperior to either of the other two in this reſpect. As far as my own obſervation goes, the Severn and Wye muſt diſembogue as much or more water than the Thames; the Humber I have not ſeen collectedly, but have noticed moſt of the branches conſtituting it, and ſhould apprehend it cannot be inferior to the Thames: all other circumſtances being the ſame, the quantity of water carried down by any river ſhould be as the area of the ground from which the water is derived, and on this account the Humber ought to exceed the Thames *.

and the diſtrict,

The Severn, which is partly derived from the mountainous country of Wales, is certainly the moſt rapid of the three rivers, and probably carries down the moſt water: as the Thames, however, is generally conſidered to take the lead, we will ſuppoſe, upon the whole, that theſe three rivers are equal in this reſpect.

which ſupply them with water.

The counties of Kent, Suſſex, Hampſhire, Dorſetſhire, Devonſhire, Cornwall, and Somerſetſhire, from the Medway to the lower Avon incluſively, in an extent of 11.000 ſquare miles, do not preſent us with many large rivers. From their number and magnitude, we cannot form a high eſtimate of their produce. The quantity of rain for thoſe counties is indeed near the average for the kingdom, as far as the preceding obſervations determine; but the milder temperature of their winters and greater heat of their ſprings and ſummers, will cauſe a greater evaporation than in ſome other parts: It is probable the rivers in theſe counties may amount, when taken together, to 1½ times the magnitude of the Thames. The rivers that diſembogue their waters on the coaſt of Lin-

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* A more perfect theorem will be given afterwards, for finding the quantity of water carried down by any river.

colnſhire, Norfolk, Suffolk and Eſſex, from the Humber to the Thames, though drawn from a country of 7000 ſquare miles, manifeſtly fall far ſhort of the Thames. The two places in this diſtrict, for which we have accounts of the rain, Norwich and Upminſter. give a mean of only 22½ inches annually. This, with the flatneſs of the country, which prevents the water from running off in ſome degree, makes the rivers much leſs than what might otherwiſe be expected from the extent of ground. There are but three or four of any conſequence, Probably all the rivers may amount to half the ſize of the Thames. There remains above 6000 ſquare miles in Wales, from the Wye to the Dee, incluſive of the laſt, and the northern counties of Lancaſter, Weſtmoreland, Cumberland, Northumberland, and Durham, with part of Cheſhire and a ſmall part of Yorkſhire, from the Merſey round by the Tweed to the Tees, amounting to 7 or 8000 ſquare miles, to be eſtimated.

The rivers upon the whole are conſidered as carrying off 13 inches out of the 36 in their fall annually.

Theſe two diviſions, though not larger than ſome others, abound in rivers, many of which are conſiderable in magnitude and of great rapidity. The rains at an average, it is probable, are double what they are in the S. E. counties of the kingdom. The rivers in theſe two diſtricts cannot fairly be eſtimated, I think, at leſs than *four* times the Thames. It appears, then, that by this eſtimation, the water carried off by all the rivers in England and Wales, may amount to *nine* times that carried off by the Thames = 13 inches of rain. There remains ſtill *ſixteen* times the water of the Thames, or 23 inches of rain to account for, before we have diſpoſed of all the rain and dew.

SECTION 3.

An Eſtimate of the Quantity of Water raiſed by Evaporation.

Evaporation

Upon looking over the ſurface of any country, three principal varieties of ſurface preſent themſelves to view, as far as reſpects evaporation, namely, *water*, ground covered with graſs and other vegetables, and bare ſoil. The difficulties that occur in attempts to find the quantity of water evaporated in thoſe three caſes, are perhaps the principal reaſon why our knowledge on this head is ſo imperfect.

from water, bare ſoil, and covered ground.

As far as experiments hitherto made authoriſe us to draw concluſions, it ſhould ſeem that the evaporation from water is greateſt; that from green ground is probably next, and that from bare ſoil the leaſt: though we may preſume, that the copious dews upon the graſs more than ſupply the exceſs of evaporation above what takes place from a moiſt uncovered ſoil.

The moſt ſatisfactory experiments I have ſeen an account of, relating to the evaporation from a ſurface of water, are thoſe of Dr. Dobſon, made at Liverpool, in the years 1772, 73, 74 and 75. (Vid. Philos. Tranſac. Vol. 67.)—He took a cylindrical veſſel of 12 inches diameter, and having nearly filled it with water, expoſed it beſides his rain-gage of the ſame aperture, and by adding water to it, or taking it away occaſionally, he kept the ſurface nearly of the ſame height, and carefully regiſtered the quantities added or taken away. by a compariſon of which with the rain, the amount of the evaporation was aſcertained. The mean monthly evaporation for four years was—January 1.50 inches.—February 1.77. —March 2.64.—April 3.30.—May 4.34.—June 4.41.—July 5.11.—Auguſt 5.01.—September 3.18.—October 2.51.—November 1.51.—December 1.49.—In all 36.78 inches. The mean rain for the ſame time was 37.48 inches.—In the year 1793 I found the evaporation from water in a ſimilar way at Kendal for 82 days in March, April, May and June to be 5.414 inches. The greateſt quantity evaporated on one of the hotteſt and drieſt days in ſummer was a little above ,2 of an inch in depth.

Dr. Dobſon's experiments on evaporation from water.

The experiments to determine how much is evaporated from green ground and from moiſt earth, are very few that have come to my knowledge. Dr. Hales, from a few experiments, calculates that moiſt earth only throws off 6$\frac{2}{3}$ inches annually.—This calculation muſt be far below the truth. Dr. Watſon, Biſhop of Llandaff, found that in a dry ſeaſon there evaporated from a graſs plat that had been mowed cloſe, about 1600 gallons in an acre per day, which amounts nearly to ,07 of an inch in depth; and that after rain the evaporation was conſiderably more. Now ſuppoſing ,07 to be the medium daily evaporation for May, June, July and Auguſt, and that as much is raiſed in theſe four months as in all the reſt of the year, the annual evaporation in ſuch circumſtances will be 17 or 18 inches, which is but half that obſerved from water at Liverpool, and ſix inches leſs than the reſerve of rain ſtated above.

Hales's eſtimate from moiſt earth.

Dr. Watſon from a graſs plat.

In

Experiment on a tube of earth to ſhew how much water ran off the ground; how much ſunk in; and how much evaporated.

In order to aſcertain this point more fully, and to inveſtigate the origin of ſprings, my friend Thomas Hoyle, jun. and ſelf, practiſed an expedient as follows, beginning in the autumn of 1795. Having got a cylindrical veſſel of tinned iron, 10 inches in diameter and three feet deep, there were inſerted into it two pipes turned downwards for the water to run off into bottles: the one pipe was near the bottom of the veſſel; the other was an inch from the top. The veſſel was filled up for a few inches with gravel and ſand, and all the reſt with good freſh ſoil. It was then put into a hole in the ground, and the ſpace around filled up with earth, except on one ſide, for the convenience of putting bottles to the two pipes; then ſome water was poured on to ſadden the earth, and as much of it as would was ſuffered to run through without notice, by which the earth might be conſidered as ſaturated with water. For ſome weeks the ſoil was kept above the level of the upper pipe, but latterly it was conſtantly a little below it, which precluded any water running off through it. Moreover, for the firſt year the ſoil at top was bare; but for the two laſt years it was covered with graſs the ſame as any green field. Things being thus circumſtanced, a regular regiſter has been kept of the quantity of rain water that ran off from the ſurface of the earth through the upper pipe (whilſt that took place) and alſo of the quantity of that which ſunk down through the three feet of earth, and ran out through the lower pipe. A rain-gage of the ſame diameter was kept cloſe by to find the quantity of rain for any correſponding time.

The

Tabulated reſults.

The following Table ſhews the Reſult.

	Water through the two Pipes. Inch. 1796.	Inch. 1797.	Inch. 1798.	Mean. inch.	Mean Rain. inch.	Mean Evap. inch.
Jan.	1.897—	,680—	1.774+	1.450+	2.458	1.008
Feb.	1.778—	,918—	1.122	1.273	1.801	,528
March	,431—	,070—	335	,279	,902	,623
April	,220—	,295—	,180	,232	1.717	1.485
May	2.027—	2,443+	,010	1.493+	4.177	2.684
June	,171—	,726—	——	,299	2.483	2.184
July	,153—	,025	——	,059	4.154	4,095
Aug.	——	——	,504	,168	3.554	3.386
Sept.	——	,976	——	,325	3.279	2.954
Oct.	——	,680	——	,227	2.899	2.672
Nov.	——	1.044	1.594	,879	2.934	2.055
Dec.	,200	3.077	1,878+	1.718+	3.202	1.484
	6.877—	10.934—	7,379	8.402	33.560	25.158
Rain	30.629—	38.791—	31,259			
Evap.	23.725—	27.857—	23.862			

Variation of the experiments.

The following obſervations were made when the water paſſed through both pipes: that is, when the veſſel was filled up with earth above the level of the upper pipe.

		Top pipe Inch.	Bottom pipe. Inch.
1796. Jan.	25	,190	,280
	30	,080	,114
Feb.	2	,100	,254
	8	,196	,140
May	1	,163	,000
	10	,060	,400
	12	,312	,175
	15	,190	,200
June	3	,120	,040
Total		1.411	1.603

Remarks on the tables.

The column of mean evaporation is derived by taking the difference of the two columns preceding it; but it ſhould be obſerved that though this method is ſufficiently exact in taking the year together, it is not ſo in taking the months ſeverally, becauſe it preſumes that the earth in the veſſel contains the ſame quantity of water at the end of each month, or is ſaturated with it; whereas in the ſummer months it is frequently ſhort of ſaturation. The conſequence is, that the evaporation appears from this table to be ſomething leſs than it really is in the ſummer months, and ſomething more in the antumnal *.

Concluſions. Evapor. from ground is 30 inches.

From theſe experiments it ſeems we may conclude—1ſt. That the quantity of water evaporated, *in the circumſtances above related*, amounts to 25 inches of rain annually; to which if we add five inches for the dew, it will give 30 inches of water raiſed annually.

It increaſes with the rain.

2d. That the quantity of evaporation increaſes with the rain, but not proportionally. Thus, 1797 gave the moſt rain and the greateſt evaporation, &c.

Deep bare ſoil and ſod do not differ.

3d. That it does not appear there is much difference betwixt the evaporation from bare earth, when there is ſufficient depth of ſoil, and that from ground covered with vegetating graſs. The account in 1796 is much what might have been expected, if the earth had been covered with graſs.

Whether this evap. of 30 and the rivers of 13, making 43 inſtead of 36 (the rain) indicate another ſupply?

As this account of evaporation, amounting to 30 inches, exceeds the medium reſerve of rain of 23 inches, it demands an enquiry whether the rain is adequate, or whether the earth derives a ſupply of water from ſome ſubterranean reſervoir, according to the opinion of ſome philoſophers.

Reply; negative.

With reſpect to the deficiency of 7 inches, there are *three* cauſes to be aſſigned for it, which appear to me fully ſufficient, without having recourſe to any ſource but that of rain for the ſupply of the earth in general.

The evapor. was taken a little too great.

1ſt. In the account of the rain that paſſed through the earth in our evaporating veſſel, there are a few monthly products marked, + thoſe were occaſioned by the bottle that received the water through the pipe being found with the water running over; this loſs was placed to the account of evaporation; it could not be much, as the water was taken ſeveral times in a month, but poſſibly might amount to one inch in the year.

The rain at Mancheſter exceeds the medium,

2d. The rain at Mancheſter, being $33\frac{1}{2}$ inches annually, exceeds the medium of 31 inches; and conſequently, according to the preceding obſervations, the evaporation ought to exceed the medium.

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* N. B. The earth in the veſſel always appeared as well ſupplied with moiſture as the ground around it, in the drieſt weather.

3d. But the principal cauſe of the exceſs in our account of evaporation, I conceive to be the prevention of the water running off from the ſurface of the earth at the top, by having the earth below the level of the upper pipe: it has been ſeen, that when the earth was above that level, a great part of the water came off that way, by which the ſurface was ſooner dried: whereas by forcing all the water to ſink through the earth or ſtand on its ſurface, a greater degree of moiſture perpetually exiſted at the ſurface, and conſequently afforded a greater ſcope for evaporation, than the ſurface of the earth in general would do.

and the water not being ſuffered to run freely off at top, did increaſe the evaporation.

Upon the whole then I think we may fairly conclude—that the *rain* and *dew* of this country are equivalent to the quantity of water carried off by evaporation and by the rivers. And as nature acts upon general laws, we ought to infer, that it muſt be the caſe in every other country, till the contrary is proved.

Hence the rain and dew fully ſupply the evaporation and the rivers.

This concluſion being admitted, we are enabled to deduce a general theorem for the quantity of water carried down into the ſea by any river in any country (on the ſuppoſition that all rivers are ramified alike) provided we have certain *data*: theſe *data* are the length of the river, and the exceſs of the rain above the evaporation in the country from which the water of the river is drawn: alſo, it ſhould be known by obſervation, how much water ſome one given river carries down.

Theorem for deducing the quantity of water carried by a river, &c.

For, from the principles of geometry, the area of country from which any river is ſupplied, will be as the ſquare of the length of the river; and the quantity of water carried off, will be in the compound ratio of the area of the country, and the exceſs of the rain and dew above the evaporation.

Thus, let $L =$ the length of any river, $E =$ the exceſs of rain and dew above the evaporation, and $Q =$ the quantity of water diſembogued in any given time by that river; $l =$ the length of any other river, $e =$ the exceſs, &c. and $q =$ the quantity of water; then we ſhall have $q = \frac{Q\,l\,e}{L\,E}$.

Statement.

Ex. gr. Suppoſe the length of the Thames = 200 miles, and the exceſs = 5 inches, eſtimating the rain and dew at 30 inches and evaporation at 25; and ſuppoſe the river Kent, in Weſtmorland,

Example.

Weſtmorland, to be 20 miles in length, and the exceſs 35 inches, the rain and dew being ſuppoſed 65, and evaporation 30 inches.

Then, $\frac{20^2 \times 35 \times Q}{200^2 \times 5} = \frac{7\,Q}{100} = q$ or $Q = 14\frac{2}{7}\,q$; which reſult, I believe, will be found to accord nearly with the meaſurement of the two rivers on the principle before mentioned.

SECTION 4.

On the Origin of Springs.

Cauſe of ſprings. The origin of ſprings has always been juſtly conſidered as a queſtion of natural hiſtory worthy of inveſtigation.—In the infancy of ſcience hypotheſes are formed to account for phenomena; but when facts are diſcovered totally inconſiſtent with an hypotheſis, it ought to be diſcarded. This does not ſeem to have been the caſe in the ſubject before us; for various opinions are ſtill held by ſome, which it is impoſſible to ſupport by facts. The object of the following remarks and experiments is to aſcertain the diſputed point if poſſible.

Opinions. There are *three* opinions reſpecting the origin of ſprings which it may be proper to notice.

1. Rain and dew. 1ſt. That they are ſupplied entirely by rain and dew.

2. Subterranean reſervoirs. 2d. That they are principally ſupplied by large ſubterranean reſervoirs of wates.

3. Filtration from the ſea. 3d. That they derive their water originally from the ſea, on the principle of filtration.

Whether the firſt cauſe be inſufficient. It is obvious, that before we pay any attention to the two latter opinions, the cauſes aſſigned in the firſt ought to be proved inſufficient by direct experiment. M. de la Hire is the only one who has attempted to do this, as far as my information extends, in the Pariſian Memoirs for 1703. He procured a leaden veſſel eight feet deep, having a pipe at the bottom; this he buried in the earth, and filled with ſoil of ſand and loam, expoſing the ſurface to receive all the rain that fell. After 15 years trial, he found that no water had run through the pipe at the bottom.

De la Hire's experiment. No rain ſoaked through a maſs of earth eight feet thick.

Shallower veſſel. Again, he took another veſſel, eight inches deep, which he filled with earth and expoſed in like manner. No rain penetrated ſo as to run out at the bottom from June to February;

but after that time it yielded a quantity after moſt rains. Another veſſel of twice the depth, or 16 inches, gave a reſult much like that of eight inches. Farther, M. de la Hire found, that when herbs were planted in the ſoil of the laſt mentioned veſſel, and grown up, no rain penetrated through the ſoil, but inſtead thereof it was not ſufficient to ſuſtain the vegetation; for the plants would require to be ſprinkled occaſionally, or elſe they began to droop and wither.

With reſpect to the firſt mentioned fact, we need not wonder that no water penetrated through eight feet of earth at Paris, where the annuul rain is but 20 inches, when only eight or nine inches penetrated through three feet of earth here, where the rain is 33 or 34 inches annually. But it does not follow that rain may not deſcend down declivities of the ground into vallies or lower parts, at Paris as well as here, and being accumulated may penetrate into the earth to a conſiderable depth, eſpecially if it meet with channels or chaſms of any kind, or declining ſtrata of earth that are impenetrable by water. Paris, I believe, however, is not very liberally ſupplied with ſprings, as might be expected. As to the experiment upon vegetation, it only proves that the rain in ſpring and ſummer is ſometimes not ſufficient to ſupport vegetable life, a fact which may readily be granted; but then in his experiment the plants were precluded from a ſupply of moiſture from the earth beneath the veſſel, which is a reſerve of the utmoſt conſequence in dry ſeaſons.

Remarks. Paris has only 20 inches annual rain;

but the higher grounds there as well as here may afford waters, &c.

This circumſtance of water aſcending again in the earth, on whatever principle it is effected, cannot be denied.—There were 4¾ inches of rain here in July laſt, none of which paſſed through the earth in the evaporating veſſel; this earth, however, at the end of the month, was far from that degree of dryneſs which is unfit for the ſupport of vegetation.—During the firſt four days of Auguſt there fell about three inches of rain, and only ½ an inch penetrated through the earth in the evaporating veſſel. Conſequently three feet in depth of earth that was moderately moiſt imbibed nearly three inches of rain before it was ſaturated; whence we may conclude that three inches nearly had aſcended and been evaporated. This evidently ſhews, that earth is capable of holding a very great proportion of water, that in ſummer the water aſcends to ſup-

Water after ſoaking into the earth aſcends again.

ply the exigencies at the ſurface, and that earth far under the point of ſaturation with moiſture is ſtill fit to ſupport vegetation.

Queſtion. How much water is contained in ſaturated ground.

This obſervation ſuggeſted the following queſtion—How much water is there in a given depth of earth when the ſoil is at the point of ſaturation, or in that ſtate when it begins to yield water from the lower pipe of the evaporating gage?

Experiment. 1 foot ſaturated earth contains 7 inches water, and it may loſe half before it is too dry for vegetation.

To determine this I took a quantity of garden ſoil that had been ſoaked with rain a day before, and preſſed it into a crucible; in this ſtate I found its ſpecific gravity to that of water as five to three. It was then expoſed to a moderate heat till it appeared, as near as I could judge, of the ſame moiſture as garden ſoil two inches deep in dry ſummer weather; afterwards it was expoſed almoſt to a red heat till it became a perfectly dry powder; in the former caſe it loſt $\frac{1}{12}$ of its weight, and in the latter $\frac{1}{3}$.—When it had loſt $\frac{1}{8}$, it did not appear too dry to ſupport vegetation. When it had loſt $\frac{2}{9}$, it appeared like the top ſoil in ſummer.—Hence it follows, that every foot of earth in depth, ſo ſaturated, contains ſeven inches of water, and that it may part with one quarter of its water, or even one half, and not be too dry for ſupporting vegetation.

Brick clay nearly the ſame.

Clay, juſt dug out for the purpoſe of making bricks, was tried in the ſame manner: It gave the ſame ſpecific gravity as the earth, and yielded not much leſs water.

De la Hire's concluſions erroneous.

Theſe experiments and obſervations prove, that M. de la Hire's concluſions, drawn from the vegetation of plants in a given quantity of ſoil, precluded from any communication with the earth at large, are erroneous, or at leaſt unwarranted: As it does not thence appear that the evaporation for the *whole* year exceeds the rain in the year, whatever it may do for a month or two in ſummer.

Springs are therefore ſupplied by the rains.

The origin of ſprings may ſtill therefore be attributed to rain, till ſome more deciſive experiments appear to the contrary; and it becomes unneceſſary to controvert the other two opinions reſpecting this ſubject.

Springs are thence deficient in ſummer.

Upon the whole it ſhould ſeem, that at the commencement of ſpring, the ground is nearly ſaturated with water for five or ſix feet in depth, as the rains and dews in autumn and winter far exceed the evaporation: There are then five or ſix inches of water at leaſt to be raiſed up again to the ſurface in caſe of exigence in the ſpring and ſummer: If this happen to be ſo, then it is at the expence of ſprings; for we find the generality of ſprings become languid, or entirely ceaſe to flow at the end of a long drought. As to the few ſprings that ſeem to be little affected by dry or wet ſeaſons, they form exceptions which it would not be difficult to account for.

VII.

Deſcription of an Inſtrument for extracting Hard Subſtances which may ſtick during their Paſſage to the Stomach. By G. C.

To Mr. NICHOLSON.

SIR,

Inſtrument for extracting bodies from the throat.

ENCLOSED, I ſend you a drawing of an untried inſtrument; if you think it likely to accompliſh the end for which it was deſigned, you will, perhaps, give it a place in your very uſeful work. This inſtrument, I conceive to be an improvement of that commonly uſed for forcing down any hard ſubſtances that may ſtick between the mouth and the ſtomach. In many caſes nails, pins, and other metallic matters, get into this ſituation, when it would undoubtedly be preferable to draw them up through the mouth, inſtead of paſſing them into the ſtomach, where they are no ſooner arrived, than they furniſh a new ſpecies of danger to the ſufferer. With this view the following inſtrument was conſtructed. A, B, Pl. XI. is a rod of whalebone, having a ſmall groove down the middle, from end to end, large enough to contain a ſtrong ſilken thread; this thread is confined to the groove by a few lappings of fine waxed ſilk. At B, is faſtened, as uſual, a ſponge, about one third of the common ſize. Juſt above the ſponge is fixed a ſmall pulley, round which the ſilken thread winds, and returning up the oppoſite ſide of the whalebone to that on which it deſcended, is tied faſt to the bottom of a ſmall leather cap C. Above this cap, at D, are faſtened 12 or 14 ſmall ſilver wires, made to ſpring into the form repreſented in Fig. 2. Theſe wires by means of loops at their ends, ſupport a round bag of net-work of fine ſilk, perforated in the centre to admit the whalebone rod. Theſe wires, together with the bag, muſt be capable of being inſerted and confined in the cap C, ſo

Deſcription.

ſo as to remain in the poſition repreſented in Fig. 1. in which ſtate it is ready for being paſſed down the oeſophagus; and it ſeems ſcarcely neceſſary to remark, that previous to the inſtrument being withdrawn, the cap C muſt be pulled off, by means of the thread at A; when the wires will expand the net, and preſs its edge cloſe to the gullet on every ſide, and in returning to the mouth, will probably bring up any ſubſtance that was lodged there, within it.

G. C.

Brompton, Jan. 23, 1803.

VIII.

On the Flexure of Wax and other Bodies by irregular cooling, with Conſiderations on the Probability that it may be cauſed by the Law of Cryſtallization. In a Letter from R. B.

To Mr. NICHOLSON.

London, Feb. 16, 1803.

SIR,

Thread of wax flowing down a candle,

A STREAM of wax has juſt overflowed the cup of the wax candle by which I have been reading, and has preſented me with a fact or two which I think worthy the meditation of philoſophers. If you think ſo, pleaſe to give them a place in your repoſitory.

becomes ſeparated by ſpontaneous flexure.

The fluid wax has formed a line or protuberance on the outſide of the candle, four inches in length, a little more than one fifteenth of an inch in width, or ſurface applied to the candle, and one tenth of an inch in elevation or thickneſs. As it grew cold it has ſeparated from the candle, ſo that its lower extremity ſtands a little more than half an inch diſtant, and it does not touch for the length of two inches and a half from the bottom; all the upper part ſtill continuing adherent. And laſtly, the ſeparated portion exhibits a regular curve, convex towards the candle, and more convex the nearer the lower extremity; ſo that, when carefully taken off, and applied to ſeveral circular arcs deſcribed upon paper, the difference was very obviouſly perceived.

The wax was cooled by different parts in *ſucceſſion.*

Upon theſe facts I obſerve from obvious reaſons that the wax which flowed in contact with the ſolid candle, was more ſpeedily cooled than that which flowed on the outſide of that heated wax; and that that portion of the wax which flowed in contact with the candle, and ran to the greateſt diſtance was the moſt ſpeedily cooled.

Deduction. Wax quickly cooled is leſs denſe than if ſlowly cooled.

And the reſult farther ſhews; 1. That the dimenſions of wax ſuddenly cooled, are larger than thoſe of wax cooled more ſlowly; and 2. The quicker the cooling the greater this difference. For the fluid wax was depoſited in a ſtrait line, and its curve figure after cooling ſhewed that the interior line or convex limit was longer then the exterior or concave limit; and this difference being greateſt where the refrigeration was moſt ſudden, namely, towards the lower extremity, was ſhewn in the greater curvature.

Apologetical remark.

I will not, Sir, do your readers the injuſtice to ſuppoſe any of them will think the dignity of philoſophy impaired by a ſpeculation on the guttering of a candle, but will proceed in my diſquiſition in hopes that others of more leiſure and ability may purſue the object farther, if found to deſerve it. And as it has been a faſhion ſince the publication of the famous ſtring of queries at the end of Sir Iſaac Newton's Optics, for ſpeculators to uſe that modeſt term to dignify inductions which they may ſuppoſe to be almoſt proved, I will take the liberty to offer a few on the preſent occaſion.

Qu. 1. Are not all bodies rarer if more ſpeedily cooled, congealed or cryſtallized?

1. Since the ſpecific gravity of ſteel ſuddenly cooled, is leſs than when annealed or ſlowly reduced in temperature; ſince ice and other cryſtals ſlowly formed, are generally underſtood to be denſer than the products of haſty refrigeration; and ſince in our experiment wax obeys the ſame law, is it not probable that the law may be general in the cooling of all bodies. *Fiant Experimenta.*

Qu. 2. Is not this cryſtallization? even in bodies already ſolid?

2. Can this effect be aſcribed to any thing but the arrangement of the parts of bodies; and if ſo, is it not referable to haſty and ſlow cryſtallization; and are we not therefore juſtifiable in ſuppoſing that the cryſtallization of bodies may be altered, even in the ſolid ſtate, as in the hardening or ſoftening of ſteel.

Qu. 3. Are not all theſe bodies more hard and leſs tenacious?

3. As the hardneſs of ſteel becomes greater and its tenacity leſs by ſudden refrigeration, is it not probable that all the products of haſty cryſtallization are harder as they are known to be more brittle. Try in ſal ammoniac whether its ſoftneſs or flexibility will become leſs by ſudden cooling, ſuch as ſubli-

mation in an head covered with ice; and examine whether an unannealed glaſs tube be not harder than a portion of the ſame tube ſlowly cooled.

Qu. 4. Will it not be uſeful to repeat this experiment with other bodies?

4. May not ſome uſeful indications as to the properties of metals and other bodies be derived from the ſimple proceſs of caſting a long ſlip of them, by pouring the fuſed metal upon a cold ſtone. If the curvature be univerſally like that of the wax, the law will be more and more confirmed. If anomalies preſent themſelves, our acquaintance with natural events will nevertheleſs be extended and improved.

Experiments.

Since writing the above, I have taken ſome of the materials next at hand to make an experiment or two, which I give without regarding whether they ſupport, modify, or deſtroy the hypotheſis advanced above.

Exp. 1. Annealed and unannealed glaſs, The annealed ſeems hardeſt.

Exp. 1. A thick glaſs tube which had been bended into a ſyphon, was broken in two at the place of flexure. It was ſuppoſed that the bended part having been heated and cooled a ſecond time, might prove ſofter from this kind of annealing. The extremity of the ſtrait part was applied to ſcratch the bended part, and alſo the other ſtrait part; and contrarywiſe the extremity of one of the bended parts was applied to the other portion of tube; and laſtly both ends were ſcratched with a file. No certainty was obtained, but it was thought that the bended part was hardeſt *.

Exp. 2. Lead caſt on a ſlab. No reſult.

Exp. 2. Lead was fuſed and poured red hot upon a marble ſlab in a long ſlip. Other lead moderately heated was poured out. The pieces did not quit the face of the ſtone in cooling, and they were too flexible to be taken up and examined.

Exp. 3. Type metal. Bended like the wax.

Exp. 3. Type metal was treated in the ſame manner. A flat piece one thirtieth of an inch thick, and ſix inches long, bended upwards from the ſtone in cooling, to the height or verſed ſine of one twentieth of an inch, and retained its flexure.

Exp. 4. Fuſible metal. Bended very much when nearly cold, *contrary to the wax, and recovered itſelf afterwards.*

Exp. 4. Fuſible metal or the compound of lead, tin, and biſmuth, was poured out in a ſlip 13 inches long, one half being about the ſame thickneſs or leſs than the type metal, and the other half conſiderably thicker. It did not appear to riſe from the ſlab; but was taken up while quite warm and held with its edge downwards, to prevent any flexure from its weight. The thin part immediately began to bend, and the thick part ſoon afterwards, *the flexures being concave on the ſide which had touched the ſtone,* and the verſed ſine or height of the curvature was one inch at the end of half a minute, after which it gradually ſtraightened itſelf as follows,

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* As the tube had been bended with the blow pipe, the difference might as well have ariſen from oxigenation of the lead or mangeneſe, or diſſipation of alkali, as from ſimple heating and cooling.

R. B.

Bar of Fuſible Metal 13 *Inches long.*

At 8^h 51^m	taken up ſtrait.		inch.
51½	it became bended;	verſed ſine	1.00
54	- - leſs bended	- do. -	0.7
55	- - ———	———	0.4
57	- - ———	———	0.25
60	thin part ſtrait; thick a little curved.		

Another experiment had been made before, without any expectation of ſuch or ſo great a flexure, and no meaſures were then taken. In this the bar was thinner and the flexure greater; and the return to ſtraitneſs was not only complete, but one end which was very thin became at laſt bended the contrary way, namely the face that had touched the ſtone was convex.

A ſmaller piece of two inches in length which was not taken up ſo ſoon, had become bended as it lay; ſo that it reſted on its two extremities and was hollow in the middle. This recovered its firſt ſtraitneſs as it lay on the table.

I am

S I R,

your obliged reader,

R. B.

IX.

An Account of ſome Caſes of the Production of Colours, not hitherto deſcribed. By THOMAS YOUNG, *M. D. F. R. S. F. L. S. Profeſſor of Natural Philoſophy, in the Royal Inſtitution* *.

Simple and general law reſpecting two portions of the ſame light arriving by different routes;

WHATEVER opinion may be entertained of the theory of light and colours which I have lately had the honour of ſubmitting to the Royal Society, it muſt at any rate be allowed that it has given birth to the diſcovery of a ſimple and general law, capable of explaining a number of the phenomena of coloured light, which, without this law, would remain inſulated and unintelligible. The law is, that "wherever two portions of "the ſame light arrive at the eye by different routes, either "exactly or very nearly in the ſame direction, the light be"comes moſt intenſe when the difference of the routes is any "multiple of a certain length, and leaſt intenſe in the inter"mediate ſtate of the interfering portions; and this length is "different for light of different colours."

which explains many phenomena,

and new facts.

I have already ſhown in detail, the ſufficiency of this law for explaining all the phenomena deſcribed in the ſecond and third books of Newton's Optics, as well as ſome others not mentioned by Newton. But it is ſtill more ſatisfactory to obſerve its conformity to other facts, which conſtitute new and diſtinct claſſes of phenomena, and which could ſcarcely have agreed ſo well with any anterior law, if that law had been erroneous or imaginary: theſe are, the colours of fibres, and the colours of mixed plates.

Production of colours by a minute fibre near the edge of an obſtacle intercepting light &c.

As I was obſerving the appearance of the fine parallel lines of light which are ſeen upon the margin of an object held near the eye, ſo as to intercept the greater part of the light of a diſtant luminous object, and which are produced by the fringes cauſed by the inflection of light already known, I obſerved that they were ſometimes accompanied by coloured fringes, much broader and more diſtinct; and I ſoon found, that theſe broader fringes were occaſioned by the accidental interpoſition of a hair. In order to make them more diſtinct, I employed a horſe-hair; but they were then no longer viſible. With a fibre of wool, on the contrary, they became very large and conſpicuous: and, with a ſingle ſilk-worm's thread, their magnitude was ſo much increaſed, that two or three of them ſeemed to occupy the whole field of view. They appeared to extend on each ſide of the candle, in the ſame order as the colours of thin plates, ſeen by tranſmitted light. It occurred to me, that their cauſe muſt be ſought in the interference of two portions of light, one reflected upon the fibre, the other bending round its oppoſite ſide, and at laſt coinciding nearly in direction with the former portion; that accordingly as both portions deviated more from a rectilinear direction, the difference of the length of their paths would become gradually greater and greater, and would conſequently produce the appearances of colour uſual in ſuch caſes; that, ſuppoſing them to be inflected at right angles, the difference would amount nearly to the diameter of the fibre, and that this difference muſt conſequently be ſmaller as the fibre became ſmaller; and, the number of fringes in a right angle becoming ſmaller, that their angular diſtances would conſequently become greater, and the whole appearance would be dilated. It was eaſy to calculate, that for the light leaſt inflected the difference of the paths would be to the diameter of the fibre, very nearly as the deviation of the ray, at any point, from the rectilinear direction, to its diſtance from the fibre.

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* Philos. Trans. 1802.

Remarks and inferences. A portion of light reflected from one ſide nearly coincides in direction with another portion inflected; but their lengths differ and produce colour.

I therefore made a rectangular hole in a card, and bent its ends ſo as to ſupport a hair parallel to the ſides of the hole; then, upon applying the eye near the hole, the hair of courſe appeared dilated by indiſtinct viſion into a ſurface, of which the breadth was determined by the diſtance of the hair and the magnitude of the hole, independently of the temporary aperture of the pupil. When the hair approached ſo near to the direction of the margin of a candle that the inflected light was ſufficiently copious to produce a ſenſible effect, the fringes began to appear; and it was eaſy to eſtimate the proportion of their breadth to the apparent breadth of the hair, acroſs the image of which they extended. I found that ſix of the brighteſt red fringes, nearly at equal diſtances, occupied the whole of that image. The breadth of the aperture was $\frac{66}{1000}$, and its diſtance from the hair $\frac{8}{10}$ of an inch: the diameter of the hair was leſs than $\frac{1}{500}$ of an inch; as nearly as I could aſcertain, it was $\frac{1}{600}$. Hence we have $\frac{11}{1000}$ for the

Preciſe repetition of the experiment

the deviation of the firſt red fringe at the diſtance $\frac{8}{10}$; and, as $\frac{8}{10} : \frac{11}{1000} :: \frac{1}{600} : \frac{11}{480000}$, or $\frac{1}{43636}$ for the difference of the routes of the red light where it was moſt intenſe. The meaſure deduced from Newton's experiments is $\frac{1}{39200}$. I thought this coincidence, with only an error of one-ninth of ſo minute a quantity, ſufficiently perfect to warrant completely the explanation of the phenomenon, and even to render a repetition of the experiment unneceſſary; for there are ſeveral circumſtances which make it difficult to calculate much more preciſely what ought to be the reſult of the meaſurement.

The halos round a diſtant candle ſeen through wool.

When a number of fibres of the ſame kind, for inſtance, a uniform lock of wool, are held near to the eye, we ſee an appearance of halos ſurrounding a diſtant candle: but their brilliancy, and even their exiſtence, depends on the uniformity of the dimenſions of the fibres; and they are larger as the fibres are ſmaller. It is obvious that they are the immediate conſequences of the coincidence of a number of fringes of the ſame ſize, which, as the fibres are arranged in all imaginable directions muſt neceſſarily ſurround the luminous object at equal diſtances on all ſides, and conſtitute circular fringes.

Coloured atmoſpheric halos.

There can be little doubt that the coloured atmoſpherical halos are of the ſame kind: their appearance muſt depend on the exiſtence of a number of particles of water, of equal dimenſions, and in a proper poſition, with reſpect to the luminary and to the eye. As there is no natural limit to the magnitude of the ſpherules of water, we may expect theſe halos to vary without limit in their diameters; and, accordingly, Mr. Jordan has obſerved that their dimenſions are exceedingly various, and has remarked that they frequently change during the time of obſervation.

New colours ſeen through two plates of glaſs with a little moiſture between them mixed with air.

I firſt noticed the colours of mixed plates, in looking at a candle through two pieces of plate-glaſs, with a little moiſture between them. I obſerved an appearance of fringes reſembling the common colours of thin plates; and, upon looking for the fringes by reflection, I found that theſe new fringes were always in the ſame direction as the other fringes, but many times larger. By examining the glaſſes with a magnifier, I perceived that wherever theſe fringes were viſible, the moiſture was intermixed with portions of air, producing an appearance ſimilar to dew. I then ſuppoſed that the origin of the colours was the ſame as that of the colours of halos; but, on

Theſe were not produced by inflection and reflection but by the different velocities of the light through water and air.

Proofs of this theory.

a more minute examination, I found that the magnitude of the portions of air and water was by no means uniform, and that the explanation was therefore inadmiſſible. It was, however, eaſy to find two portions of light ſufficient for the production of theſe fringes; for, the light tranſmitted through the water, moving in it with a velocity different from that of the light paſſing through the interſtices filled only with air, the two portions would interfere with each other, and produce effects of colour according to the general law. The ratio of the velocities in water and in air, is that of 3 to 4; the fringes ought therefore to appear where the thickneſs is 6 times as great as that which correſponds to the ſame colour in the common caſe of thin plates; and, upon making the experiment with a plane glaſs and a lens ſlightly convex, I found the ſixth dark circle actually of the ſame diameter as the firſt in the new fringes. The colours are alſo very eaſily produced, when butter or tallow is ſubſtituted for water; and the rings then become ſmaller, on account of the greater refractive denſity of the oils: but, when water is added, ſo as to fill up the interſtices of the oil, the rings are very much enlarged; for here the difference only of the velocities in water and in oil is to be conſidered, and this is much ſmaller than the difference between air and water. All theſe circumſtances are ſufficient to ſatisfy us with reſpect to the truth of the explanation; and it is ſtill more confirmed by the effect of inclining the plates to the direction of the light; for then, inſtead of dilating, like the colours of thin plates, theſe rings contract: and this is the obvious conſequence of an increaſe of the length of the paths of the light, which now traverſes both mediums obliquely; and the effect is every where the ſame as that of a thicker plate.

The curved figure of the particles cauſe much of the light to deviate and modify the effects.

It muſt however be obſerved, that the colours are not produced in the whole light that is tranſmitted through the mediums: a ſmall portion only of each pencil, paſſing through the water contiguous to the edges of the particle, is ſufficiently coincident with the light tranſmitted by the neighbouring portions of air, to produce the neceſſary interference; and it is eaſy to ſhow that, on account of the natural concavity of the ſurface of each portion of the fluid adhering to the two pieces of glaſs, a conſiderable portion of the light which is beginning to paſs through the water will be diſſipated laterally by reflection

tion at its entrance, and that much of the light paſſing through the air will be ſcattered by refraction at the ſecond ſurface. For theſe reaſons, the fringes are ſeen when the plates are not directly interpoſed between the eye and the luminous object; and, on account of the abſence of foreign light, even more diſtinctly than when they are in the ſame right line with that object. And, if we remove the plates to a conſiderable diſtance out of this line, the rings are ſtill viſible, and become larger than before; for here the actual route of the light paſſing through the air, is longer than that of the light paſſing more obliquely through the water, and the difference in the times of paſſage is leſſened. It is however impoſſible to be quite confident with reſpect to the cauſes of theſe minute variations, without ſome means of aſcertaining accurately the forms of the diſſipating ſurfaces.

The arguments from the general law ſhew that the velocity of undulation is greateſt in rare mediums.

The central black ſpot on a ſoap bubble is produced by undulations reflected from the confines of a denſe and a rare medium in circumſtances to deſtroy each other;

a contrary effect is produced when both reflections are from a rarer medium.

In applying the general law of interference to theſe colours, as well as to thoſe of thin plates already known, I muſt confeſs that it is impoſſible to avoid another ſuppoſition, which is a part of the undulatory theory, that is, that the velocity of light is the greater, the rarer the medium; and that there is alſo a condition annexed to the explanation of the colours of thin plates, which involves another part of the ſame theory, that is, that where one of the portions of light has been reflected at the ſurface of a rarer medium, it muſt be ſuppoſed to be retarded one half of the appropriate interval, for inſtance, in the central black ſpot of a ſoap-bubble, where the actual lengths of the paths very nearly coincide, but the effect is the ſame as if one of the portions had been ſo retarded as to deſtroy the other. From conſidering the nature of this circumſtance, I ventured to predict, that if the two reflections were of the ſame kind, made at the ſurfaces of a thin plate, of a denſity intermediate between the denſities of the mediums containing it, the effect would be reverſed, and the central ſpot, inſtead of black, would become white; and I have now the pleaſure of ſtating, that I have fully verified this prediction, by interpoſing a drop of oil of ſaſſafraſs between a priſm of flint-glaſs and a lens of crown glaſs: the central ſpot ſeen by reflected light was white, and ſurrounded by a dark ring. It was however neceſſary to uſe ſome force, in order to produce a contact ſufficiently intimate; and the white ſpot differed, even at laſt, in the ſame degree from perfect whiteneſs, as the black ſpot uſually does from perfect blackneſs.

The

Diſperſion of colours by refraction explained from the aſſumptions that light paſſes through the particles and through the pores.

The colours of mixed plates ſuggeſt to me an idea which appears to lead to an explanation of the diſperſion of colours by refraction, more ſimple and ſatisfactory than that which I advanced in the laſt Bakerian lecture. We may ſuppoſe that every refractive medium tranſmits the undulations conſtituting light in two ſeparate portions, one paſſing through its ultimate particles, and the other through its pores; and that theſe portions re-unite continually, after each ſucceſſive ſeparation, the one having preceded the other by a very minute but conſtant interval, depending on the regular arrangement of the particles of a homogenous medium. Now, if theſe two portions were always equal, each point of the undulations reſulting from their re-union, would always be found half way between the places of the correſponding point in the ſeparate portions; but, ſuppoſing the preceding portion to be the ſmaller, the newly combined undulation will be leſs advanced than if both had been equal, and the difference of its place will depend, not only on the difference of the length of the two routes, which will be conſtant for all the undulations, but alſo on the law and magnitude of thoſe undulations; ſo that the larger undulations will be ſomewhat further advanced after each reunion than the ſmaller ones, and, the ſame operation recurring at every particle of the medium, the whole progreſs of the larger undulations will be more rapid than that of the ſmaller; hence the deviation, in conſequence of the retardation of the motion of light in a denſer medium, will of courſe be greater for the ſmaller than for the larger undulations. Aſſuming the law of the harmonic curve for the motions of the particles, we might without much difficulty reduce this conjecture to a compariſon with experiment; but it would be neceſſary, in order to warrant our concluſions, to be provided with very accurate meaſures of the refractive and diſperſive powers of various ſubſtances, for rays of all deſcriptions.

Compariſon of Dr. Wollaſton's obſ. with the ſeparation of colours by thin plates.

Dr. Wollaſton's very intereſting obſervations would furniſh great aſſiſtance in this inquiry, when compared with the ſeparation of colours by thin plates. I have repeated his experiments on the ſpectrum with perfect ſucceſs, and have made ſome attempts to procure comparative meaſures from thin plates; and I have found that, as Sir Iſaac Newton has already obſerved, the blue and violet light is more diſperſed by refraction, than in proportion to the difference of the appropriate dimenſions deduced from the phenomena of thin plates. Hence it happens, that when a line of the light proceeding to form an image of the rings of colours of thin plates, is intercepted by a priſm, and an actual picture is formed, reſembling the ſcale delineated by Newton from theory, for eſtimating the colours of particles of given dimenſions, the oblique ſpectrums, formed by the different colours of each ſeries, are not ſtraight, but curved, the lateral refraction of the priſm ſeparating the violet end more widely than the red. The thickneſs correſponding to the extreme red, the line of yellow, bright green, bright blue, and extreme violet, I found to be inverſely as the numbers 27, 30, 35, 40, and 45, reſpectively. In conſequence of Dr. Wollaſton's correction of the deſcription of the priſmatic ſpectrum, compared with theſe obſervations, it becomes neceſſary to modify the ſuppoſition that I advanced in the laſt Bakerian lecture, reſpecting the proportions of the ſympathetic fibres of the retina; ſubſtituting red, green, and violet, for red, yellow, and blue, and the numbers 7, 6, and 5, for 8, 7, and 6.

Subdiviſion of the light of a candle explained.

The ſame priſmatic analyſis of the colours of thin plates, appears to furniſh a ſatisfactory explanation of the ſubdiviſion of the light of the lower part of a candle: for, in fact, the light tranſmitted through every part of a thin plate, is divided in a ſimilar manner into diſtinct portions, increaſing in number with the thickneſs of the plate, until they become too minute to be viſible. At the thickneſs correſponding to the ninth or tenth portion of red light, the number of portions of different colours is five; and their proportions, as exhibited by refraction, are nearly the ſame as in the light of a candle, the violet being the broadeſt. We have only to ſuppoſe each particle of tallow to be, at its firſt evaporation, of ſuch dimenſions as to produce the ſame effect as the thin plate of air at this point, where it is about $\frac{1}{10000}$ of an inch in thickneſs, and to reflect, or perhaps rather to tranſmit, the mixed light produced by the incipient combuſtion around it, and we ſhall have a light completely reſembling that which Dr. Wollaſton has obſerved. There appears to be alſo a fine line of ſtrong yellow light, ſeparate from the general ſpectrum, principally derived from the moſt ſuperficial combuſtion at the margin of the flame, and increaſing in quantity as the flame aſcends. Similar circumſtances might undoubtedly be found in other caſes of the production or modification of light; and experiments upon this ſubject might tend greatly to eſtabliſh the Newtonian opinion, that the colours of all natural bodies are ſimilar in their origin to thoſe of thin plates; an opinion which appears to do the higheſt honour to the ſagacity of its author, and indeed to form a very conſiderable ſtep in our advances towards an acquaintance with the intimate conſtitution and arrangement of material ſubſtances.

Diſperſive powers of the eye.

I have lately had an opportunity of confirming my former obſervations on the diſperſive powers of the eye. I find that at the reſpective diſtances of 10 and 15 inches, the extreme red and extreme violet rays are ſimilarly refracted, the difference being expreſſed by a focal length of 30 inches. Now the interval between red and yellow is about one-fourth of the whole ſpectrum; conſequently, a focal length of 120 inches expreſſes a power equivalent to the diſperſion of the red and yellow, and this differs but little from 132, which was the reſult of the obſervation already deſcribed. I do not know that theſe experiments are more accurate than the former one; but I have repeated them ſeveral times under different circumſtances, and I have no doubt but the diſperſion of coloured light in the human eye is nearly ſuch as I have ſtated it. How it happens to be no greater, I cannot at preſent undertake to explain.

X.

A Memoir on the Wax Tree of Louiſiana and Pennſylvania. By CHARLES LOUIS CADET, *of the College of Pharmacy at Paris*[*].

Wax bearing plants.

A GREAT number of plants, ſuch as the *croton ſebiferum*, the *tomex ſebiſera* of *Loureiro*, the poplar, the alder, the pine, and ſeveral labiated plants, afford a concrete inflammable matter by decoction, more or leſs reſembling tallow or wax, that is to ſay, a fixed oil ſaturated with oxigen. The light matter, which is called the down of fruits, which ſilvers the ſurface of prunes and other ſtone fruits, is wax, as Mr. Prouſt has ſhewn. But the tree which preſents this ſubſtance

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[*] Tranſlated from the Ann. de Chimie, XLIV. 140.

The moſt productive is the myrica cerifera.

in the greateſt abundance, and in more reſpects than one, is entitled to the attention of cultivators, is the *myrica cerifera*, or wax tree.

Its early hiſtory.

We read in the Hiſtory of the Academy of Sciences for the years 1722 and 1725, that M. Alexandre, ſurgeon, correſpondent with M. Marian, had obſerved at Louiſiana a tree of the ſize of a cherry tree, having the appearance of the myrtle, and bearing a grain of the ſize of coriander ſeed. Theſe grains, of a grey aſh colour, contain a ſmall round hard kernel, which is covered with a ſhining wax that may be obtained by boiling the grains in water. This wax is drier and more friable than ours. The inhabitants make candles of it. M. Alexandre adds, "This grain is uſually of a deep and beautiful lake colour, which by merely cruſhing with the fingers leaves them tinged, but this is only at a particular ſeaſon."

The liquor in which the grain has been boiled, and from whence the wax is procured, having been poured out and evaporated to the conſiſtence of an extract, M. Alexandre diſcovered that it checks the moſt obſtinate dyſenteries.

The advantageous properties that this tree appears to poſſeſs, ought to have induced philoſophers to make enquiries to aſcertain the various properties of the vegetable, and what attention its culture might require; it has been long conſidered merely as an object of curioſity.

Linnæus, in his vegetable ſyſtem, only mentions the wax tree of Virginia, myrica cerifera, the leaves lanceolated as if dentated, and the ſtem arboreſcent.

Species of the myrica cerifera.

On enquiring of Cit. Ventenat, whether there were ſeveral ſpecies, he informed me, that Ayton diſtinguiſhes two, viz.

1. The *myrica cerifera anguſtifolia*, which grows in Louiſiana. This tree is very delicate, it flowers with difficulty in our green houſes; and its grains are ſmaller than thoſe of the following.

2. The *myrica cerifera latifolia*, which grows in Penſylvania, Carolina, and Virginia, does not riſe ſo high as the former; it is perfectly naturalized in France. Theſe two myrica are diæcous.

They are both cultivated in the *Muſéum des Plantes*, and in the gardens of Citizens Cels and Lemonier (at Paris.)

Cit. Michault admits of a third ſpecies of *myrica cerifera*, which he calls the dwarf wax tree. Cit. Ventenat believes that wax may be procured from all the myrica.

Authors who have written reſpecting it.

The authors who have mentioned theſe trees with ſome detail are Marſhal, tranſlated (into French) by Leſerme, Lepage-Duprat, and Toſcan, librarian at the Muſeum of Natural Hiſtory. A memoir inſerted by the laſt in his work intitled *L'Ami de la Nature*, ſhews the manner in which the vegetable wax is collected in the colonies.

Manner of collecting the wax in America.

"Towards the end of autumn," ſays he, "when the berries are ripe, a man leaves his houſe, together with his family, to go to ſome iſland or bank on the ſea ſhore where the wax trees grows in abundance. He carries with him veſſels to boil the berries, and a hatchet to build a cottage where he may find ſhelter during his reſidence in this place, which is uſually three or four weeks. While he cuts down the trees his children gather the berries. A very fertile ſhrub will afford near ſeven pounds. When theſe are gathered the whole family employ themſelves in procuring the wax. They throw a certain quantity of berries into the kettle, and then pour a ſufficient quantity of water on them ſo as to cover them to a depth of about half a foot. They boil the whole, ſtirring the grains about and rubbing them againſt the ſides of the veſſel, in order that the wax may more eaſily come off. In a ſhort time it floats on the water like fat, and is collected with a ſpoon and ſtrained through a coarſe cloth to ſeparate it from any impurities which might be mixed with it. When no more wax can be obtained, they take the berries out with a ſkimmer, and put others into the ſame water; but it muſt be entirely changed the ſecond or third time, and in the mean time boiling water muſt be added as it evaporates, in order to avoid retarding the operation. When a conſiderable quantity of wax has been obtained by this means, it is laid upon a cloth to drain off the water with which it is ſtill mixed. It is then dried and melted a ſecond time, to render it more pure, and it is then formed into maſſes. Four pounds of berries afford about one of wax; that which is firſt obtained is generally yellow: but in the latter boilings it aſſumes a green colour from the pellicle with which the kernel of the berry is covered."

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It is there uſed for ſoap making.

The traveller Kalm, ſpeaking of the vegetable wax, ſays, that in the country where the wax tree grows they make excellent ſoap of it, which waſhes linen perfectly white.

The author obtains ſome of the wax;

Such are the notions which have been formed reſpecting the myrica; at leaſt no other obſervations had been publiſhed to my knowledge, when a naturaliſt favoured me with half a kilogramme (17½ oz. avoird.) of the vegetable wax of Louiſiana. I was deſirous of making a comparative analyſis of it with bees wax; but before I undertook this work, I wiſhed to ſee the tree and berry of the myrica. I ſaw this precious vegetable at the *Jardin des Plantes*, and I wrote to Cit. Deſhayes, a zealous botaniſt, who cultivates the myrica penſylvania at Rambouillet, to requeſt him to give me ſome information concerning it. He had the politeneſs to ſend me an anſwer with ſome of the berries, which I immediately examined.

and examines the grain.

This grain is a kind of berry of the ſize of a pepper corn; the outſide when it is ripe and freſh, is white, and covered with ſmall black aſperities which give it the appearance of ſhagreen. When it is rubbed in the hands, it renders them greaſy or unctuous.

Experiments to ſeparate the waxy part mechanically.

If one of theſe little berries be ſtrongly preſſed, it parts with a matter reſembling ſtarch, and mixed with ſmall brown round grains like fine gunpowder. The nut which remains bare has a very thick ligneous covering, and contains a dycotildonous kernel. By rubbing a handful of the berries on a ſieve of horſe hair, I obtained a grey powder, in which the eye diſtinguiſhes without the aſſiſtance of a magnifying glaſs, the ſmall brown grains I mentioned, mixed with a white powder.

Application of alcohol.

I put this powder into alcohol, which with the aſſiſtance of a ſlight heat, diſſolved all the white part, and left the black powder which I ſeparated. Water poured on this ſolution in alcohol formed a precipitate that floated on the ſurface of the liquid. I melted this and obtained a yellow wax reſembling that ſent me from Louiſiana. This experiment compleatly proves that the wax of the myrica is the white grumous matter that ſurrounds the berries.

Black powder from the ſurface of the berries.

The black powder which was ſeparated appeared to me to contain the colouring principle, and I hoped alſo to find in it the beautiful lake mentioned by M. Alexandre. With this notion I cruſhed the powder ſtrongly, and boiled it in a ſolution of acid ſulphate of alumine; but was very much ſurpriſed to obtain nothing but a liquor ſcarcely coloured, and by which the alumine, when precipitated by an alcali, was but ſlightly tinged.

I took another portion of this fine black powder, and infuſed it in alcohol, I ſoon obtained a tincture of the colour of wine lees, which on being heated, became as red as a ſtrong tincture of quinquina, or terra japonica. The reſult led me to think that the colouring principle was reſinous; but on adding water to it, I did not perceive that any precipitate was formed.

I then poured into this tincture water charged with ſulphate of alumina, and obtained ſlight precipitate. A ſolution of ſulphate of iron immediately formed ink.

The powder afforded an aſtringent ſolution by alcohol.

What may this aſtringent colouring principle be which is only ſoluble in alcohol, which is not precipitated by water, and has ſo little attraction for alumine? To diſcover it, it is neceſſary to make a courſe of experiments which the ſmall quantity of the ſubſtance I was in poſſeſſion of would not permit me to undertake. The aſtringent matter mentioned by M. Alexandre muſt be found in the decoction of the entire grain. To aſcertain this fact I boiled ſome in a ſilver ſaucepan, the decoction upon which floated a little of the wax, was of a greeniſh colour, its taſte ſlightly ſtyptic, and it precipitated black ferruginous ſolutions. I heated it in an iron veſſel for that purpoſe, and it immediately became black. To diſcover whether this property aroſe from the gallic acid alone, or from tanin, I mixed a little of the concentrated decoction with a ſolution of gelatine, but it afforded no precipitate.

It contained gallic acid but no tanin,

It is therefore to the conſiderable quantity of gallic acid which the berries of the myrica contain, that we muſt attribute its effect in dyſenteries. I therefore ſuppoſe that the bark and leaves of the tree muſt contain an extract much more aſtringent than the berries.

Examination of the wax.

The examination of the wax afforded extremely intereſting reſults.

In whatever manner the wax may be obtained; by the decoction of the grains, or the ſolution of the powder when precipitated from alcohol by water, this wax when melted is al-

It is greeniſh yellow; firm, and more brittle than bees wax, burns well in candles, and emits a fragrant ſmell.

In diſtillation it is affected like bees wax.

ways of a greeniſh yellow. It is of a firmer conſiſtence than bees wax, it is dry, and ſufficiently friable to be pulverized. In a word, it is evidently more oxigenated than the wax prepared by bees. Candles made of the wax of the myrica afford a white flame, a good light, without ſmoke, and do not gutter; they emit when quite freſh a balſamic odour, which the inhabitants of Louiſiana conſider as extremely wholeſome for perſons in ill health. When diſtilled in a retort this wax paſſes over for the moſt part in the form of butter. This portion is much whiter, and has no more conſiſtence than tallow. Another portion that was decompoſed afforded a little water, with ſome empyreumatic oil, and ſebacic acid. Much carbonated hidrogen gas, and carbonic acid gas were diſengaged; there remained in the retort a black and coaly bitumen. Wax uſually is affected in the ſame manner by diſtillation.

Ether diſſolves it better than alcohol.

I have already mentioned, that alcohol diſſolves the wax of the myrica, but ether diſſolves it better, and it ſeparates in the form of ſtalagmites by the evaporation of the liquid; neither of them diſcolour it. If this wax be boiled with weak ſulphuric acid it becomes paler, but there is no evident combination of the acid with it. The yellow bees wax under the ſame circumſtances did not change colour.

Ox. mur. acid bleaches it.

Oxigenated muriatic acid renders both kinds of wax perfectly white. The vegetable wax is the moſt difficult to be bleached.

It is ſoluble in ammonia;

Vegetable wax diſſolves in ammonia: the ſolution is of a brown colour; a portion of the wax is ſaponified. Volatile alkali has much leſs action on bees wax.

and forms ſoap with fixed alkali.

Both kinds of wax when ſtrongly agitated in a boiling ſolution of cauſtic potaſh, become white and form a real ſoap as Kalm obſerved.

Its whiteneſs in ſoap.

The whiteneſs acquired by the wax during this ſaponification is not a new phenomenon. Chaptal in his proceſs for bleaching by the vapor of alkaline leys, proves that the colouring principle of vegetables yields to the action of alkalis. Some chemiſts have attributed this effect to the direct combination of the ſoda or potaſh with the coloured extract, a combination which makes it almoſt of a ſoapy quality, and renders it ſoluble.

Chemical obſervation on the oxigenation of the oil in ſoap.

I apprehend that in this operation, the alkali exerciſes a double attraction on the oil or wax, firſt directly with the conſtituent principles of the oil and then prediſpoſing, and favouring the combination of the atmoſpheric oxigen with the oil or wax. I do not know whether my notion is original; but I deduced it from the obſervation of what paſſes during the decompoſition of a ſoap by an acid: the oil is always concrete and becomes oxigenated. It would be intereſting for chemical theory to make, if it were poſſible, a ſoap in a cloſed apparatus the air of which might be examined after the experiment, or in different gaſes not containing oxigen.

The veg. wax from its ſoap is in a peculiar ſtate.

When the ſoap of myrica is decompoſed a very white wax is obtained, but in a peculiar ſtate rendering it unfit for our uſes.

Plaiſters with veg. wax.

Litharge or the ſemi-vitreous oxide of lead diſſolves very well in melted wax of Louiſiana; it forms a very hard plaiſter, but its conſiſtence may be diminiſhed at pleaſure by the addition of a little oil. If as there is reaſon to ſuppoſe, the wax of myrica retains a portion of the aſtringent principle afforded by the decoction of the berries, phyſicians may perhaps diſcover ſome uſeful topical remedies in the compounds of this wax.

Great utility of this wax.

From what has been ſaid, we ſee that the myrica may be of the greateſt uſe to the arts. The wax which it affords is ſufficiently abundant to recompenſe the care and expenſe of cultivating it. For a buſh in a full bearing yields from ſix to ſeven pounds of kernels, one fourth of which may be obtained in wax. It is ſuperior in quality to bees-wax.

and of its aſtringent matter.

The aſtringent principle of the myrica, extracted in the large way may be very uſeful in medicine and in the arts; it may to a certain extent be ſubſtituted inſtead of nut galls in dyeing, hat making, and probably in certain proceſſes of tanning. The colouring principle appears ſufficiently ſolid to deſerve ſome attention; and if it be true that ſome fine lakes have been obtained from it in Louiſiana, why may we not expect advantages from it in painting.

Laſtly when this wax ſhall have become plentiful and cheap in the market it promiſes great advantage in the fabrication of ſoap.

To bleach veg. wax.

The art of bleaching this wax will alſo require a courſe of experimental reſearch; if it be propoſed to operate economically and in the large way. Two re-agents preſent themſelves to manufacturers; the ſulphuric acid, and the oxigenated muriatic acid. But as the wax does not ſink in theſe liquids, we muſt multiply the contacts either by ſlicing and ſprinkling it with the oxigenated muriatic acid, or by incloſing it in this divided ſtate in caſks into which the oxigenated muriatic gas ſhould be paſſed.

The preference given to the action of the ox. mur. of lime.

I ſhall propoſe a third which promiſes a ſpeedier effect. The wax in a very divided ſtate is to be ſtratified in a caſk, with ſuper-oxigenated muriate of lime; in this manner they are to be diſpoſed in ſtrata, and left for ſome time in contact without moiſture. The ſalt is afterwards to be decompoſed with water acidulated by the ſulphuric acid; taking care to pour the water a little at a time at different intervals, until there ſhall be no longer any perceptible diſengagement of muriatic gas; at which period a large quantity of water is to be added and the mixture agitated with a ſtaff. The inſoluble ſulphate of lime falls down by repoſe, while the bleached wax riſes and ſwims at the ſurface. This is to be waſhed and melted on the water bath.

On the culture of the tree in Europe.

I ſhall conclude this memoir by offering ſome notions reſpecting the culture of the *myrica penſylvanica*.

Cit. Deſhayes to whom I am indebted for the trials I have made has obſerved the wax tree for ſeveral years at Rambouillet. The following is what he writes to me on the ſubject.

It is perfectly at home in France.

"The *myrica (latifolia)* Ayton is here abſolutely in its native country; it is in the ſoil beſt ſuited to it, namely in a ſandy and blackiſh turf: we have ſixteen very flouriſhing wax trees. Their height is from four to five and ſix feet, and one of them a male is ſeven feet high. The berries are abundant almoſt every year, I ſay almoſt becauſe there are ſome years in which they have failed. In general this fruit thrives very well in the part of the Engliſh garden aſſigned to it.

and requires no care in its culture.

"The culture requires no care. Every year numerous ſhoots are taken off which riſe at the feet of the large wax trees. Theſe are planted in other parts of the garden at the diſtance of one metre or yard aſunder."

The grain may be ſowed in the ground in ſpring, and afterwards tranſplanted, but the proceſs would be too long. The myrica ſucceeds wherever the ſoil is light and rather moiſt. How many provinces are there in which this cultivation might become uſeful and give employment to lands at preſent nearly abandoned.

It even grows well in Pruſſia.

How great are the advantages our agriculture might expect from this acquiſition, ſince the myrica has long ago been ſeen to flouriſh in the dry ſands of Pruſſia. Cit. Thieboult of the academy of Berlin, has communicated this intereſting fact to me in the following note.

Narrative of a plant in the garden of Mr. Sulzer.

The late Mr. Sulzer author of the general dictionary of the fine arts, obtained from Frederic the Great a portion of waſte ground of conſiderable extent on the banks of the Spree, half a league from Berlin, in a place called the Moabites. However ungrateful this ſoil might appear as it preſented only a very ſcanty and thin turf, upon a fine and light ſand, Mr. Sulzer converted it into a garden very agreeable and worthy of a philoſopher. Among other remarkable things, he made a plantation of foreign trees compoſed of five long rows, in the direction from Eaſt to Weſt. There were not two trees in ſucceſſion of the ſame ſpecies; he had placed in the rows moſt expoſed to the North, none but ſuch as were loftieſt and moſt capable of reſiſting the rigours of the climate, So that by proceeding from North to South, the firſt row preſented only trees of about ſeventy feet in height, the ſecond trees between twenty-five and thirty feet high, and ſo in ſucceſſion in an amphitheatre, where all the trees enjoyed the ſun, at leaſt in a part, and the weakeſt were ſheltered by thoſe which were more hardy.

It was in the ſouthernmoſt row, that I obſerved a kind of buſh only two or three feet high, which Mr. Sulzer called the wax tree. All the viſitors took particular notice of this tree in preference to all others, on account of the delicious odour of its leaves, which they preſerved a very long time.

Citizen Thieboult afterwards ſpeaks of the extraction of the wax. This operation does not differ from that related by Mr. Alexander.

Remarkable perfume from the candles of veg. wax.

I have ſeen, adds he afterwards, a ſingle candle of this wax perfume the three chambers which compoſed the particular apartment of Mr. Sulzer, not only during the time it was lighted, but alſo for the reſt of the evening.

Without doubt the myrica cultivated at Berlin, was more odoriferous than that which grows with us, for ours does not emit the ſame perfume. Mr. Sulzer had the project of making candles of this wax, not bleached, but covered with our wax for the ſake of beauty. The heirs of the academician have ſold the garden, but the wax tree ſtill remained. It was planted in 1770. Since the poſſibility of naturalizing the *myrica cerifera* has been aſcertained in the north, why ſhould we neglect a vegetable of ſuch value and importance which could not fail to proſper in our ſouthern departments, and demands much leſs care than our bee-hives. The ſucceſsful experiments already made ought to excite the zeal of our cultivators.

Economical Remarks.

The government has already encouraged this branch of induſtry, by ordering plantations to be made. There exiſts at Orleans and at Rambouillet, two orchards of the wax tree which contain more than four hundred ſhrubs. We cannot give too much publicity to ſuch ſatisfactory reſults. Nothing is propagated with ſo much ſlowneſs as uſeful plants. A barren but pictureſque tree, or an agreeable flower are ſoon adopted by the faſhion. They ornament the gardens of our modern Lucullufes, and the toilets of our Phrynes, while our indefatigable agriculturiſts make vain efforts to enrich our gardens with a new gramineous plant, or to fill our barns with nouriſhing cereal plants. The people has long rejected from prejudice both maize and the potatoe which have been ſo highly ſerviceable to the poor and to our ſoldiery. We no longer find in our foreſts the food bearing oak, upon which our anceſtors ſubſiſted. Let us hope that our cultivators will at laſt open their eyes upon their true intereſts, and that leſs enſlaved by old practices they will not deſpiſe the preſents which learned ſocieties are deſirous of making for their profit, and the reputation and proſperity of their country.

XI.

Outline of the Craniognomic Syſtem of Dr. Gall[*], *Phyſician at Vienna. By Dr.* BOJAMES[†].

Various ſyſtems of phyſiognomy,

THE deſire of finding in the external ſtructure of man, certain indications of his internal faculties, his paſſions, his moral diſpoſition, &c. has in the moſt remote as well as in modern times, engaged philoſophers to eſtabliſh ſyſtems of phyſiognomy, which have been more or leſs ſatisfactory.

of Porta, of Lavater, and of the facial angle.

The moſt generally known are thoſe of Porta, Lavater, the theory of the facial angle, and laſtly the ſyſtem of Gall.

Syſtem of Porta, haſty and inaccurate.

With regard to the firſt, who has buſied himſelf in comparing the outlines of the figure of man, with that of brutes, obſervers have decided on its value, and conſider his principles as the product of a wild imagination; they have found them too haſty, very little eſtabliſhed on reaſonable obſervations, and abſolutely uncertain in application.

Lavater's ſyſtem founded on ſentiment, not ſcience.

Lavater's ſyſtem has had more ſucceſs; but though we revere the genius of this man who was really a great obſerver, we cannot be ignorant of the looſe foundation on which all his opinions are built, and the mind is unſatisfied with truths which can only be appreciated by an imagination as exalted, and feelings as delicate as thoſe of the author.

Theory of the facial angle true; but too general.

The theory of the facial angle which comprehends a more ample field than the ſyſtem of Lavater, leaves us in uncertainty as to the detail of faculties, and gives us only general points of view. But it preſents this moſt important truth, that the facial angle increaſes in magnitude in equal proportion with the faculties of animals, and in this point it evidently agrees with the general reſults of Gall's ſyſtem.

Syſtem of Gall its fundamental principles.

Without entering into a ſcrupulous detail of the laborious courſe which this learned philoſopher has followed, in order to eſtabliſh certain baſes for a ſcience hitherto ſo hypothetical, I ſhall content myſelf with a ſhort examination of its fundamental principles, which are,

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[*] This hiſtorical expoſition which does not in the leaſt tend to prove the truths of Gall's ſyſtem, ſhould not influence any one's judgment concerning it, as it will be confirmed by its author with ſolid reaſoning and convincing proofs.

It is likewiſe neceſſary to remark, that the ſentences marked with inverted commas do not reſt on the authority of Gall.

[†] Inſerted by him in the Encyclopedie Methodique, of the learned *Millin.*

1ſt. *The Brain is the material Organ of the Internal Faculties.*

The brain is the organ of the faculties.

Without endeavouring to decide upon the metaphyſical queſtions reſpecting the nature of the ſoul, or of that which may be ſuppoſed the occult cauſe of the internal faculties, we are nevertheleſs compelled to admit a material organ for their action.

Proofs.

Now when we remark that theſe faculties are found only where the brain exiſts, that they are loſt with it; that the diſorders and injuries ſuſtained by this organ, very ſenſibly influence their degree and their action; that the volume of the brain increaſes in direct proportion with the faculties of animals, &c. when we obſerve all this I ſay, there is nothing of conjecture in ſuppoſing the brain to be their material and intermediate organ.

Note. It might here be objected that in many caſes individuals have loſt a conſiderable portion of the ſubſtance of the brain, without their faculties having been ſenſibly diminiſhed; but it muſt be obſerved, that in general the organs of the brain are double, and that the caſes are far from being accurately ſtated or eſtabliſhed.

The brain conſiſts of independent organs.

2d. *The Brain contains different Organs independent* of each another, for the different Faculties.*

The internal faculties do not always exiſt in equal proportions with reſpect to each other; there are men who have much intelligence without much memory, courage without circumſpection, and metaphyſical genius without being profound obſervers.

Proofs. The faculties are exerted independently of each other, &c.

Again the phenomena of dreams, of ſomnambuliſm, of madneſs, &c. prove that the internal faculties do not always act together, that ſome are often extremely active, while others are at the ſame time totally inſenſible.

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* This notion of independence does not deſtroy the principle of animal organization, that all the parts have a mutual relation; it only indicates that the action of one organ does not abſolutely cauſe the ſame degree in another. B.

Thus in old age, and ſometimes in diſeaſes, madneſs for example, many faculties are loſt whilſt others ſubſiſt; and conſtant employment of the ſame faculty ſenſibly diminiſhes its energy; if we paſs to another we find it has all the force of which it is ſuſceptible, and when we again employ the firſt faculty, we find it has recovered its original vigour. Thus it is that when fatigued with an abſtract and philoſophical reading, we turn with pleaſure to poetry, and then afterwards apply again with equal attention to our former employ.

Theſe phenomena prove that the faculties are ſeparated and independent of one another, and we are induced to believe that the ſame is the caſe with their material organs.

Theological note.

Note. "We do not entirely adopt this notion of Gall, but on the contrary, we believe that the ſeparation of the material organs muſt be conſidered as the cauſe of the diſtinction of the internal faculties; at leaſt it ſeems to us, that by ſuppoſing them originally ſeparated, we cannot avoid the ſnare of the materialiſm which exiſts as ſoon as we ceaſe to conſider the ſpirit (l'eſprit) as unity."

3d. *The Developement of the Organs contained in the Cranium, is in direct Proportion with the Force of their correſponding Faculties.*

The organs of the brain are more developed the ſtronger the correſpondent faculties,

This principle dictated by analogy, depends on the axiom; that through all nature the faculties are found to be ever in proportion to their relative organs, and its truth is eminently proved by the particular obſervations of Gall.

and the more from exerciſe.

It muſt however be obſerved, that exerciſe has a conſiderable influence on the force of the faculties, and that an organ but moderately developed, but which is often exerciſed, may afford a ſuperior faculty to that which accompanies an organ of great magnitude, but is never put in action. Thus we ſee men whoſe ſtructure is but moderately ſtrong, acquire by continual exerciſe powers ſuperior to others whoſe ſtructure is almoſt athletic.

A large brain does not indicate energy of faculties unleſs the organs be ſeparately developed.

Note. I muſt here anticipate an opinion which ſeems to reſult immediately from this principle, but which is nevertheleſs falſe, that the volume of the brain is in direct proportion to the energy of its faculties. Obſervation has demonſtrated to Gall, that the power of the faculties can only be appreciated by the developement of the organs ſeparately, which form diſtinct eminences on the cranium, and that a cranium perfectly round, whatever may be its magnitude, never exhibits faculties either great or numerous.

Conjecture as to the reaſon.

I do not recollect that I ever heard Gall give his reaſons for the concluſion: "but I think we may conſider theſe heads in a ſtate ſimilar to that of obeſity, and as we do not judge of the muſcular ſtrength of a man or animal by the volume of its members, but by their particular developement, ſo I think we muſt judge of the force of the faculties by the developement of their reſpective organs."

The external figure of the cranium exhibits the organs and ſhews the power of the faculties, becauſe the form of the cranium depends on that of the brain.

Laſtly, the fourth principle which is the moſt important in the practice of the ſyſtem of Gall, is: *That we may judge of theſe different organs and their faculties by the exterior form of the cranium.*

The truth of this principle is founded on another; namely, that the formation of the cranium depends on that of the brain, a truth generally known and proved by the anteriority of the brain, and by the impreſſions or indentations within the cranium.

Remark.

Note. It is true that there are craniums in which there is an internal protuberance of the bone correſponding with the external projection; and this irregularity, which is ſometimes found to be a diſorder, moſt commonly in an advanced age, when the organs of the brain do not ſo powerfully reſiſt the cranium, throws a degree of uncertainty in the practice of the ſyſtem of Gall; but this is the fate of all our truths which are dictated by experience: theſe caſes, however, are not very frequent.

On theſe principles Gall has compared the organs of men and animals,

Guided by theſe principles Gall has examined nature; he has compared the craniums of men and animals, and thoſe of men of ſimilar and of different faculties. His reſearches have almoſt inconteſtibly proved, not only the truths above ſtated, but that the faculties of animals are ſimilar to thoſe of man; that what we call inſtinct in animals is alſo found in man; for example, love, cunning, circumſpection, courage, &c. that the quantity of organs is the ſtandard which fixes the generic difference of animals; their mutual proportion, that of the individuals; that the diſpoſition to any faculty which is originally given by nature, may be developed by exerciſe and favourable circumſtances, and ſometimes by diſorders; but that it can never be created if not given by nature*; that the accumulation of organs is conſtantly made from back to front, and from below upwards, ſo that animals in proportion as they reſemble man in the quantity of their faculties, have the ſuperior and anterior part of the cranium more developed; and laſtly in man, the moſt perfect of animals, there are organs in the ſuperior and anterior parts of the frontal and parietal bones aſſigned to faculties which excluſively belong to him. "In this laſt point of view it is that the diſcoveries of Gall perfectly coincide with the theory of the facial angle, which appears to confirm their truth."

Detail of the ſyſtem.

It is difficult to give an accurate and ſatisfactory account of the detail of this ſyſtem, and of the various organs which Dr. Gall has diſcovered, without ſtating the variety of facts and examples which he exhibits as evidence to prove what he advances; I will however undertake this enumeration, being perſuaded that it will in many reſpects ſhew the author's manner of reaſoning, and give a clear notion of the manner of proceeding required to obtain his reſults †.

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1. *The Organ of Tenacity of Life.*

Organs.
1. Tenacity of life.

The firſt organ which the author thinks he has diſcovered is that of the tenacity of life, *tenacitas vitæ;* he ſuppoſes the *medulla oblongata* to be its ſeat; and as the circumference of the great aperture of the *occiput* is in direct proportion to the extent of the *medulla oblongata*, he judges of the intenſity of the life of an animal by the magnitude of this perforation.

The obſervations which ſupport this opinion are, that the perforation is generally larger in the *cranium* of women than men; that it is conſtantly of greater extent in the cat, the otter, the beaver, the badger, &c. animals which are known to be extremely tenacious of life. And the moſt ſpeedy method of killing an animal is to divide the ſpinal marrow.

2. *Organ of Inſtinct for ſelf-preſervation.*

2. Self-preſervation.

Forwarder than the *medulla oblongata* at the place where it quits the brain, the author ſuppoſes the *organ of love of life, or inſtinct for ſelf-preſervation* to exiſt.

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* It is neceſſary that the germ of any organ ſhould ſubſiſt in the embryo, in order to its ſubſequent developement as an organ.

† Compare the correſponding numbers with thoſe of Plate X.

As animals do not afford examples of ſuicide, he could only obtain proofs of this ſuppoſition in the human race, and ſeveral caſes of voluntary ſuicide in which this part of the brain was diſordered, have induced him to conſider it as the organ of this faculty; he does not however admit it as indiſputably eſtabliſhed, but waits for further examples to confirm the fact.

3. *Organ of the choice of Food.*

3. Choice of food.

The author ſuppoſes the organs for the choice of food to be placed in the quadrijumal tubercles, the anterior of which are greater in carnivorous animals, the poſterior developed in the herbivoran, but are of equal ſize in the omnivorous animals.

4. *Cerebral Organs of the external Senſes.*

4. External ſenſes.

The middle part of the baſe of the brain is appropriated to the external ſenſes. It is the region from which thoſe nerves iſſue which are diſtributed into the organs of thoſe ſenſes.

(To be continued.)

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Account of the Ventriloquiſm exhibited by M. FITZ-JAMES.

Account of ventriloquiſm.

THE reader will doubtleſs recollect ſome curious facts and obſervations on ventriloquiſm by Mr. Gough in our ſecond volume, page 125. That acute philoſopher, reaſoning from a few facts and ſtating the want of more, appears inclined to adopt the theory that the art of ventriloquiſm conſiſts in cauſing the voice to iſſue from the mouth only, and uttering it in ſuch a direction, that the hearer may receive the impreſſion of ſome echo with conſiderably more force than he receives the original ſound. He gives inſtances of this proceſs, particularly where the ſound of a ring of bells appears to change its direction accordingly, as the hearer by moving along, receives it from different reflecting ſurfaces, while the original ſound is interrupted by ſome obſtacle. Whether the echos in a room be at all likely to be ſo managed would admit of conſiderable doubt; and without having witneſſed any exhibition of this kind in the leaſt to be compared with the ſurprizing narratives we occaſionally hear, I have always been ſtrongly diſpoſed

Account of ve triloquiſm.

diſpoſed to think that the deluſions with regard to the ſuppoſed direction of the voice in ſuch caſes are not phyſical but moral; that is to ſay, they have ariſen from ſomething in the nature of the ſubject, or the poſition and action of the ſpeaker, with the character, tone, and manner of ſpeaking. On the preſent occaſion I have the ſatisfaction to give ſome account of the performance of M. Fitz. James, one of the firſt maſters of this art; who in addition to his very ſtriking powers as a ſpeaker and actor has the candour and liberality to explain the nature of his performance to his auditors. I was preſent a few evenings ago at a public exhibition, which continues to be repeated at *Dulau's* in Soho-ſquare; and though my account of what I ſaw and heard cannot but be very imperfect, and far from exciting the ſurprize which the actual performance produces, it may nevertheleſs be of utility to eſtabliſh a few principles, and remove ſome errors reſpecting this art.

After a comic piece had been read by Monſ. Volange, M. Fitz-James, who was ſitting among the audience, went forward and expreſſed his ſuſpicion, that the ventriloquiſm was to be performed by the voices of perſons concealed under a platform which was covered with green cloth. Replies were given to his obſervations apparently from beneath that ſtage; and he followed the voices with the action and manner of a perſon whoſe curioſity was much excited, making remarks in his own voice, and anſwering rapidly and immediately in a voice which no one would have aſcribed to him. He then addreſſed a buſt which appeared to anſwer his queſtions in character, and after converſing with another buſt in the ſame manner, he turned round, and in a neat and perſpicuous ſpeech explained the nature of the ſubject of our attention: and from what he ſtated and exhibited before us, it appeared that by long practice he had acquired the faculty of ſpeaking during the inſpiration of the breath with nearly the ſame articulation, though not ſo loud nor ſo variouſly modulated as the ordinary voice formed by expiration of the air. The unuſual voice being formed in the cavity of the lungs is very different in effect from the other. Perhaps it may iſſue in a great meaſure through the trunk of the individual. We ſhould ſcarcely be diſpoſed to aſcribe any definite direction to it; and conſequently are readily led to ſuppoſe it to come from the place beſt adapted to what was ſaid. So that when he went to the

door

ccount of ven- iloquiſm.

door and * aſked " Are you there," to a perſon ſuppoſed to be in the paſſage, the anſwer in the unuſual voice was immediately aſcribed by the audience to a perſon actually in the paſſage; and upon ſhutting the door and withdrawing from it, when he turned round, directing his voice to the door and ſaid, " Stay there till I call you," the anſwer which was lower, and well adapted to the ſuppoſed diſtance and obſtacle interpoſed, appeared ſtill more ſtrikingly to be out of the room. He then looked up to the cieling and called out in his own voice, " What are you doing above? do you intend to come down?" to which an immediate anſwer was given, which ſeemed to be in the room above, " I am coming down directly." The ſame deception was practiſed on the ſuppoſition of a perſon being under the floor, who anſwered in the unuſual but a very different voice from the other, that he was down in the cellar putting away ſome wine. An excellent deception of the watchman crying the hour in the ſtreet, and approaching nearer the houſe till he came oppoſite the window was practiſed. Our attention was directed to the ſtreet by the marked attention which Fitz-James himſelf appeared to pay to the ſound. He threw up the ſaſh and aſked the hour, which was immediately anſwered in the ſame tone, but clearer and louder; but upon his ſhutting the window down again the watchman proceeded leſs audibly, and all at once the voice became very faint, and Fitz-James in his natural voice ſaid, " he has turned the corner." In all theſe inſtances, as well as others, which were exhibited to the very great entertainment and ſurprize of the ſpectators, the acute obſerver will perceive that the direction of the ſound was imaginary, and aroſe entirely from the well ſtudied and ſkilful combinations of the performer. Other ſcenes which were to follow required the imagination to be too completely miſled to admit of the actor being ſeen. He went behind a folding ſcreen in one corner of the room, when he counterfeited the knocking at a door. One perſon called from within, and was anſwered by a different perſon from without, who was admitted, and we found from the converſation of the parties, that the latter was in pain, and deſirous of having a tooth extracted. The dialogue, and all the particulars of the operation that followed, would require a long diſcourſe if I were to attempt to deſcribe them to the reader. The imitation of the

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* The whole performance was in French.

the natural and modulated voice of the operator encouraging, ſoothing, and talking with his patient; the confuſion, terror, and apprehenſion of the ſufferer; the inarticulate noiſes produced by the chairs and apparatus, upon the whole, conſtituted a maſs of ſound which produced a ſtrange but comic effect. Looſe obſervers would not have heſitated to aſſert, that they heard more than one voice at a time; and though this certainly could not be the caſe, and it did not appear ſo to me, yet the tranſitions were ſo inſtantaneous without the leaſt pauſe between them, that the notion might very eaſily be generated. The removal of the ſcreen ſatisfied the ſpectators that one performer had effected the whole.

Account of ventriloquiſm.

The actor then proceeded to ſhew us ſpecimens of his art as a mimic; and here the power he had acquired over the muſcles of his face was fully as ſtrange as the modulations of his voice. In ſeveral inſtances he cauſed the oppoſite muſcles to act differently from each other; ſo that while one ſide of his face expreſſed mirth and laughter, the other ſide appeared to be weeping. About eight or ten faces were ſhewn to us in ſucceſſion as he came from behind the ſcreen, which together with the general habits and gait of the individual totally altered him. In one inſtance he was tall, thin, and melancholic; and the inſtant afterwards, with no greater interval of time than to paſs round behind the ſcreen, he appeared bloated with obeſity, and ſtaggering with fulneſs. The ſame man at one time exhibited his face ſimple, unaffected, and void of character, and the next moment it was covered with wrinkles expreſſing ſlyneſs, mirth, and whim of different deſcriptions. How far this diſcipline may be eaſy or difficult, I know not, but he certainly appeared to me to be far ſuperior to the moſt practiſed maſters of the countenance I have ever ſeen.

During this exhibition he imitated the ſound of an organ; the ringing of a bell, the noiſes produced by the great hydraulic machine of Marli, and the opening and ſhutting of a ſnuff box.

His principal performance, however, conſiſted in the debates at the meeting of *Nauterre*, in which there were twenty different ſpeakers, as is aſſerted in his advertiſement; and certainly the number of different voices was very great. Much entertainment was afforded by the ſubject, which was

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Account of ventriloquiſm.

taken from the late times of anarchy and convulſion in France; when the loweſt, the moſt ignorant part of ſociety, was called upon to decide the fate of a whole people by the energies of folly and brute violence. The ſame remark may be applied to this debate, as to the other ſcene reſpecting tooth-drawing; namely, that the quick and ſudden tranſitions, and the great differences in the voices gave the audience various notions, as well with regard to the number of ſpeakers, as to their poſitions and the direction of their voices.

This account of a very celebrated ventriloquiſt may perhaps ſeem too minute and particular for a philoſophical Journal, which is leſs devoted to do juſtice to the talents of men, than to inveſtigate the cauſes of things. But where a ſtriking deluſion may lead to miſtaken theories of ſound, I conceive it to be no ſmall part of the argument, which is to ſet the truth in a clear light, to ſhew how numerous and extraordinary the acquiſitions and the ability of the performer may be. There is likewiſe a point of delicacy ariſing from the ſuſpicion of improper motives, when a public performer is ſpoken of in terms of approval by a periodical writer. On this head, however, I feel ſo little difficulty, that I am almoſt in doubt whether I ſhould obliterate the laſt obſervation, or ſuffer it to paſs.

Aſcent of Mont Blanc and Mont Perdu.

Aſcent of Mont Blanc by M. Forneret and Dortheren,

M. Forneret of Lauſanne, and the Baron de Dortheren, have undertaken a new excurſion to the ſummit of Mont Blanc. After two days travel, they arrived at the ſummit, where the wind was ſo turbulent that they were forced to ſit together with their guides in a maſs to prevent their being precipitated. The cold was ſix degrees below congelation, and with the rarity of the air affected their lungs in ſo painful a manner, that they declare that no inducement ſhould prevail on them to repeat this expedition. The enterprize was without any beneficial conſequence to the ſciences. *Bibliotheque de Sonini.*

by de Sauſſure,

It was in the year 1787, that the celebrated De Sauſſure, arrived at the ſummit of Mont Blanc; and his voyage was conſidered as a real conqueſt for the natural ſciences. Sonini has learned by a private letter, that the indefatigable Ramond, well known from his reſearches in natural hiſtory, has at laſt reached the ſummit of Mont Perdu, the giant of the Pyreneans,

of Mont Perdu by Ramond.

neans as Mont Blanc is that of the Alps. This mountain covered with glaciers and eternal ſnows, which is elevated more than eighteen hundred toiſes above the level of the ſea; and riſes higher than all the granitic Pics except Mont Blanc, muſt nevertheleſs be ranked among ternary mountains, ſince it contains the remains of ſea animals and quadrupeds. The efforts of Ramond to reach the ſummit have till now been conſtantly unſucceſsful, and he is the only one who has yet accompliſhed it. It preſents on all ſides threatening projections and ſteep precipices. This time, inſtead of directing his courſe from north to ſouth by aſcending the mountain on the French ſide, he travelled from ſouth to north by the ſlope which is directed towards Spain. He will ſoon publiſh the intereſting recital of his aſcenſion.

Experiment on Sound.

Low Sounds ſaid to be conveyed to great diſtances.

In the Journal laſt quoted, there is an account or notice of an experiment performed in the palace of the Tribunat, which as there is ſome obſcurity probably owing to the conciſeneſs of the narrative, I tranſlate literally.

Two figures of the ſize of life placed at the extremity of a very extenſive apartment, hear ſpeaking with a very low voice in whatever poſition and however diſtant the perſons who ſpeak to them may be, and they anſwer all queſtions in the moſt ſatisfactory manner.

This curious experiment proves to philoſophers, that it is poſſible to aſſiſt the organ of hearing in the ſame manner as inſtruments have been contrived to aſſiſt the ſight.

The author affirms, that it is poſſible to tranſmit intire diſcourſes uttered in a low voice to very remote diſtances, without any indication of the place of the ſpeaker.

Evaporation of Water at an elevated Temperature.

Experiments on evaporation.

M. Leindenfroſt, in a diſſertation publiſhed in 1756, announced, that water loſes the quality of evaporating accordingly as the heat of the bodies upon which it falls, is augmented from the point of ebullition to the temperature of a white heat. A drop of water which he let fall into an iron ladle heated to whiteneſs, was firſt divided into ſeveral ſmall globules which afterwards united in one. Conſidering this with attention, he obſerved, that it turned with great ſwift-

neſs

Experiments on evaporation

neſs on its axis, and became ſmaller and ſmaller. After 34 or 35 ſeconds it diſappeared with noiſe. A ſecond drop which fell into the ſame ladle now ſomewhat cooled, diſappeared in nine or ten ſeconds; laſtly, a third diſappeared in three ſeconds. When the ſpherule of water in the white hot ladle was touched with a cold body, it diſappeared in an inſtant.

Klaproth has repeated theſe experiments in the following manner. He took a highly poliſhed iron ladle which he heated to whiteneſs, and threw therein a drop of water, which became divided into ſeveral globules of different ſizes, and ſoon united into a ſingle ſphere. As ſoon as this globule which exhibited the phenomena before deſcribed, had diſappeared, he let fall a ſecond, after which a third, &c. and he obſerved, that the duration of each was leſs the more the ladle was cooled. The following is the reſult of two experiments. In the firſt experiment the intenſity of the heat being greater than in the ſecond, the degree at which the water moſt ſpeedily evaporates happened later.

1ſt *Experiment.*	ſeconds.	2d *Experiment.*	ſeconds.
The firſt drop laſted	40	The firſt drop laſted - -	40
The ſecond - - -	20	The ſecond - - - -	14
The third - - -	6	The third - - - - -	2
The fourth - - -	4	The fourth - - - -	1
The fifth - - - -	2	The fifth - - - - -	0
The ſixth - - -	0		

Theſe experiments require to be made with much care. M. Klaproth obſerves, that the ſlighteſt circumſtances produce variations in the duration of the drops.

Seven drops having been ſucceſſively thrown into the ladle heated to the neceſſary degree, ſoon united into a globular maſs, which began its movement by a very rapid rotation. This maſs afterwards divided at top, when a ſpot of white froth was ſeen at the upper part of the ball, and its edges appeared indented.

This experiment was made with ten drops, and afforded the ſame reſult; but when a greater number was employed, the maſs was unable to preſerve its rotatory motion, and the whole of the water diſappeared with a hiſſing noiſe.

If inſtead of the iron ladle or ſpoon, a capſule of pure ſilver or of platina heated to whiteneſs be uſed, the appearances are nearly the ſame, but the duration of the balls is commonly longer.

Dr. Wollaston, on the oblique refraction of Iceland Crystal.

Fig. 1.

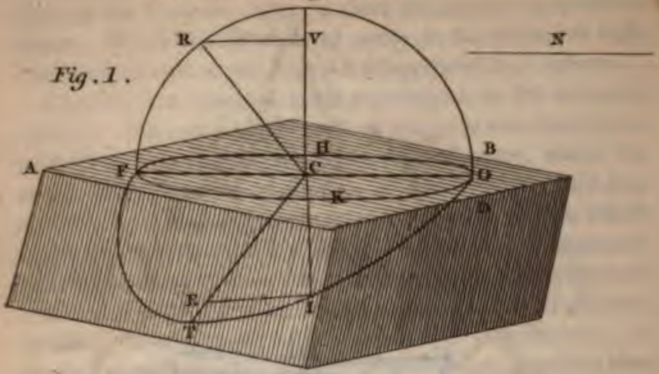


Fig. 2.

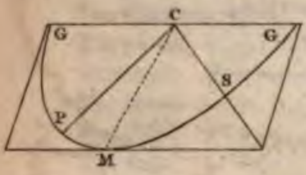


Fig. 3.

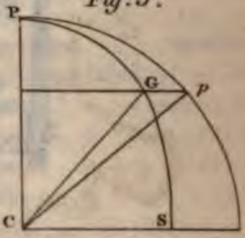


Fig. 4.

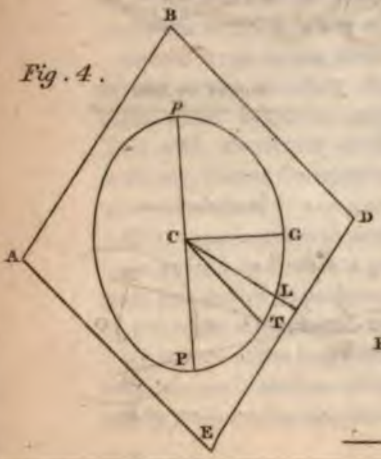
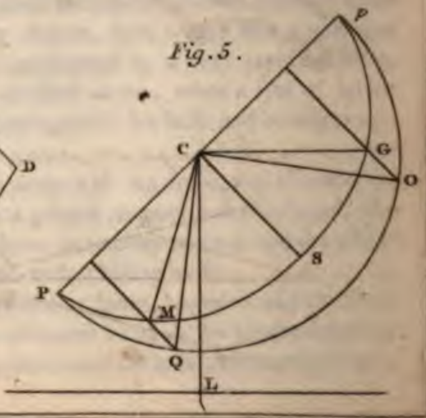
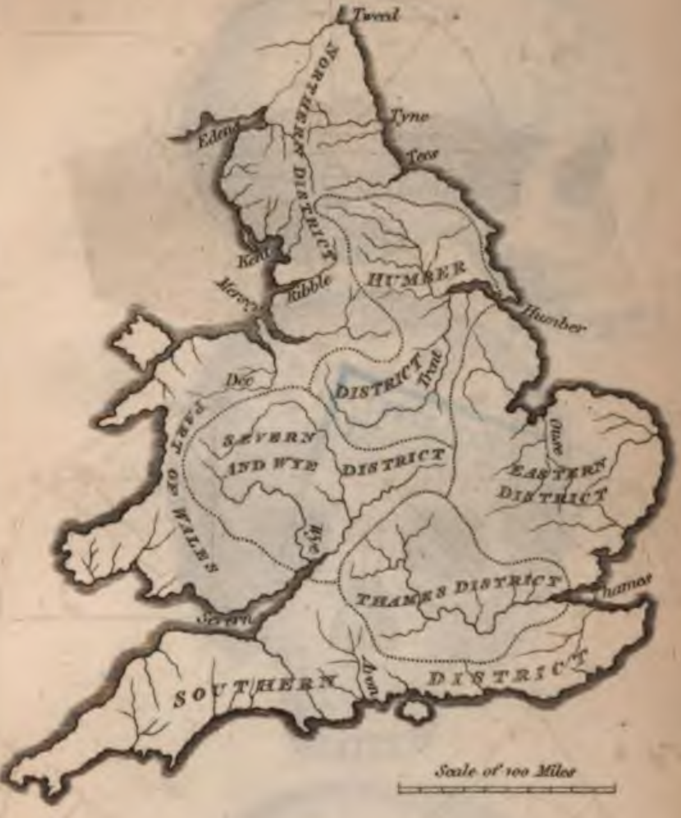


Fig. 5.



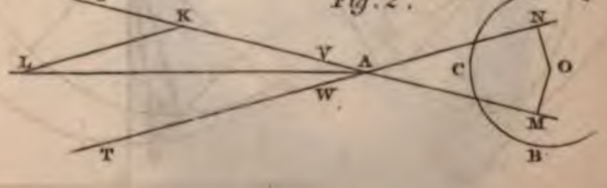
Mutlow Sc. Russell Cot.

Mr. Dalton's Sketch of the Rivers in England & Wales, divided into districts.



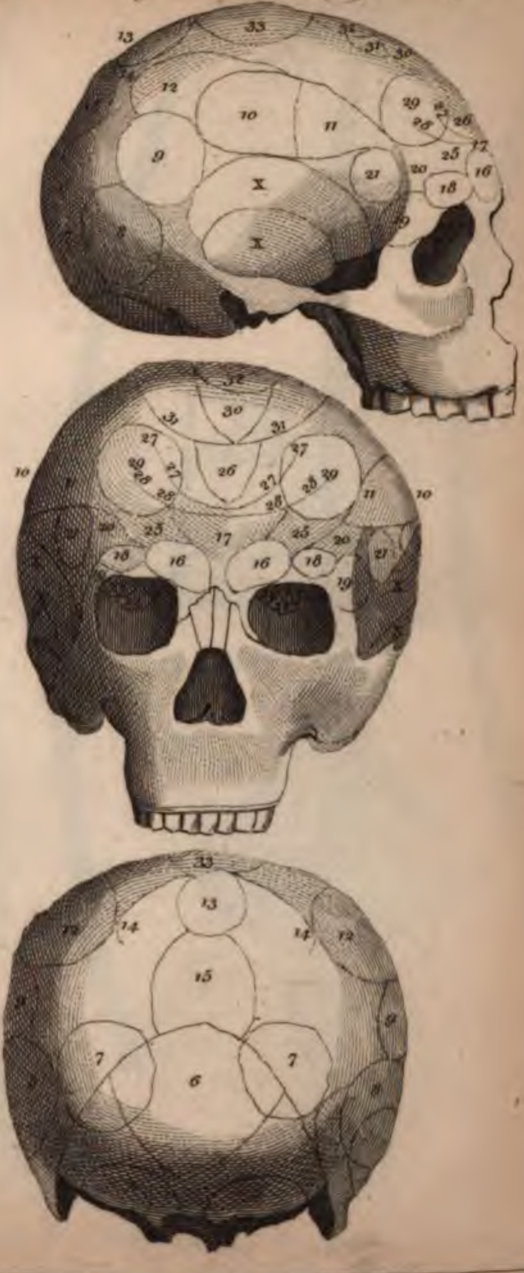
Mr. Gough on sonorous Undulation.

Fig. 2.



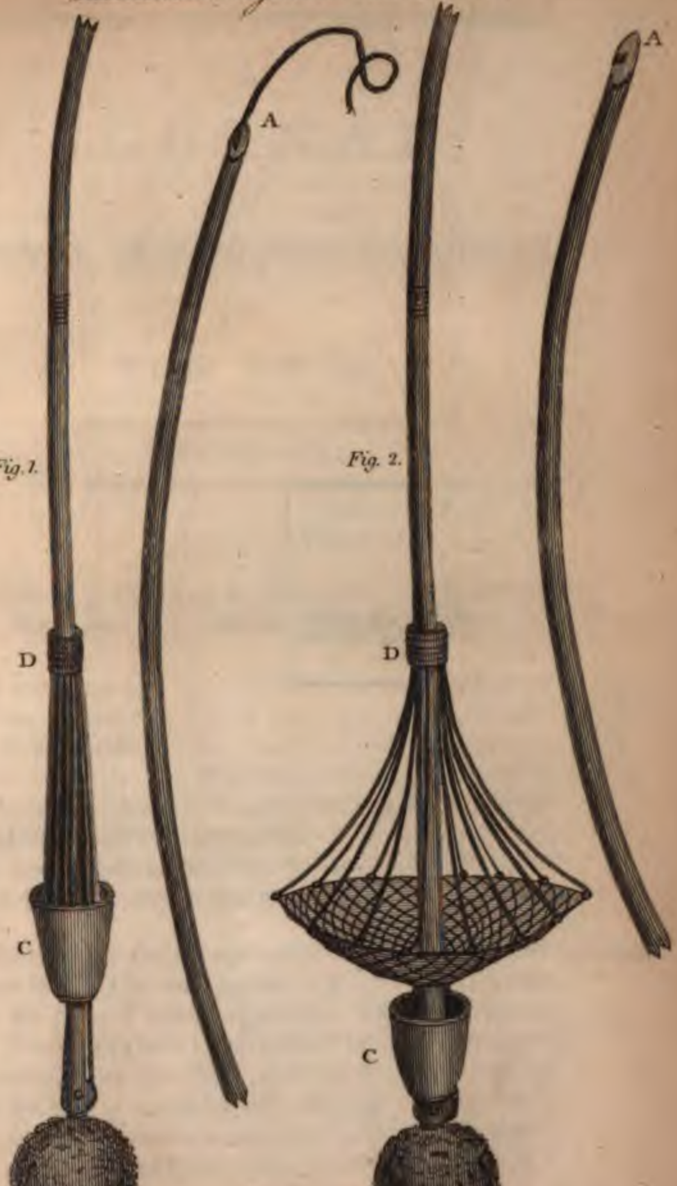
Mutlow Sc. Boſwell Ct.

The craniognomic System of Dr Gall, of Vienna.



Mutlow Sc. Ruſsell Co.

Instrument for extracting hard substances lodged in the Throat.



Mutlow Sc. Russell

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

APRIL, 1803

ARTICLE I.

An Analyſis of a Variety of the Corundum. By the Reverend WM. GREGOR. *Communicated by the Author.*

History of the ſpecimen of corundum; from Thibet.

THE mineral, which is the ſubject of the following obſervations, was preſented to me by my valuable and reſpected friend Philip Raſhleigh, eſq. who was ſtruck with the peculiarity of its colour. Mr. Raſhleigh informed me, that he had found its ſpecific gravity to be 3,6 at the temperature 60, and my experiments gave the ſame reſult.

The country where this ſtone was found, is Thibet: but in what particular part of that extenſive region I am not informed.

Characters.

"The colour of this ſtone is motled: the prevailing tint is a diluted lilac. The maſs appears to be a confuſed cryſtalliſation, the parts of which are unequal, but moſtly very minute. The larger grains reflect a luſtre not inferior to that of the common adamantine ſpar. Although the adheſion of the parts of the maſs is not conſiderable, the parts themſelves have a great degree of induration, and from the ſame cauſe, probably, a greater ſpecific gravity than the maſs itſelf," which I found to be 3,603.

A.

Analyſis, pulverization, ignition, trituration.

100 grains of this ſtone, which had been reduced to a powder in a ſteel mortar, were not affected in the ſlighteſt degree by the magnet.

The powder was of a lilac inclining to a pink-colour. Expoſed to a ſtrong red heat for half an hour, it loſt $\frac{15}{16}$ of a grain in weight. Its colour was unaltered.—It was now rubbed (in a dry ſtate) to a finer powder in a mortar of flint. Upon weighing it again, I found that it had gained no acceſſion to its weight from the abraſion of it.

Boiled to dryneſs in potaſh; then waſhed; and the reſidue again treated and waſhed; and again, &c. till little remained.

It was now put into a ſilver-crucible, and covered with a ſolution of potaſh in * alkohol, mixed with an equal bulk of diſtilled water, and the crucible was placed in a ſand-bath, and the fluid gradually evaporated: and at laſt it was boiled to dryneſs. The ſalt which remained at the bottom of the crucible, was diſſolved in diſtilled water, and its ſolution poured off from a ſpungy earth, and a portion of the undecompoſed ſtone which had ſubſided. This operation was repeated in the ſame way with freſh portions of potaſh, until the whole of the ſtone was decompoſed, except a ſmall quantity of a ſpungy earth, which was thrown upon a filtre and waſhed with diſtilled water, until what paſſed through it ceaſed to cauſe the leaſt turbidneſs in a ſolution of nitrat of mercury. The edulcorating water was added to the ſolution which had been effected by potaſh.

B.

The undecompoſed reſidue was partly diſſolved by ſulphuric acid.

The powder which remained on the filtre was dried. It was of a greyiſh white colour. I put it into a ſmall matraſs and moiſtened it with diſtilled water, and then dropped ſome

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* I have reaſon to ſubſcribe to the opinion of Mr. Chenevix, which he has given us in the maſterly and ſcientific paper on the arſeniats of copper, with which he has enriched the annals of mineralogy and chemical analyſis, that it is by the means of alkohol alone, that potaſh can be prepared, which is fit for delicate experiments. I was induced to employ a ſolution of potaſh in alkohol, becauſe it has been ſaid, that during the proceſs for obtaining it in a dry ſtate, it acts in ſome degree upon the ſilver veſſels, in which its ſolution is evaporated. By evaporating it *in contact with the ſpar*, I thought that this would be leſs likely to happen.

rectified ſulphuric acid upon it, and placed the veſſel in a digeſting heat. The powder became at firſt gelatinous. But this inconvenience was gradually removed, by the further addition of acid. What was diſſolved thereby was extracted by diſtilled water.

The laſt reſidue was again treated with potaſh, and its reſidue by ſulphuric acid. The ſolutions were added to the former, and a ſmall portion of ſilex remained.

The earth which remained, after having been ſufficiently edulcorated, was dried and expoſed to the action of potaſh in a ſilver crucible as before, and what the potaſh diſſolved was added to the alkaline ſolution before mentioned (A *b*). What was undiſſolved by it was heated again with ſulphuric acid, until it ceaſed to act upon it. The ſoluble part was extracted by diſtilled water. The reſiduum was thrown on a filtre and waſhed with diſtilled water till it ceaſed to affect nitrat of mercury: dried and heated red hot for half an hour, it weighed $\frac{15}{8}$ gr. It was pure ſilex.

The ſulph. ſolution was precip. by ammonia. It contained no lime.

The ſolution effected by ſulphuric acid, was mixed with the edulcorating water and decompoſed by ammonia. A white ſpongy earth was precipitated. When the ammonia ceaſed to cauſe any further precipitation, the clear decanted fluid was aſſayed with carbonat of ammonia and carbonat of potaſh, but no change was produced on it by the ſolution of either of thoſe ſalts. From which circumſtance the abſence of lime may be inferred.

The waſhed ammon. precip. again treated with potaſh; in part diſſolved.

The precipitate produced by ammonia was ſufficiently waſhed with diſtilled water, and in a moiſt ſtate, was boiled with the ſolution of potaſh in a ſilver crucible as before. The alkali diſſolved a portion of it. Which ſolution, together with the edulcorating water, which waſhed the remainder, was added to the alkaline ſolution before mentioned, (A *b*). What remained was thrown on a filtre, and edulcorated as before: dried and heated red hot for ten minutes, it weighed four grains.

It left oxide of titanium.

It was of the colour of pounded reſin. Expoſed to the flame of the blowpipe or charcoal it was unaltered. Moiſtened with melted tallow and ignited, it was not attracted by the magnet. It was taken up by a globule of the phoſphat of ammonia and ſoda, and ſuſpended in it in white flakes. It is ſoluble in the three mineral acids. But the ſolution, on being boiled, becomes turbid from the ſeparation of a large portion of it. It is precipitated from its ſolution in acids by pruſſiat * of potaſh, of a beautiful graſs-green, and by tincture of galls of a dark orange-colour. It poſſeſſed, in ſhort, all the properties of the oxide of titanium.

C.

The alk. ſolutions ſat. with mur. acid, and diſſolved; precip. with carb. of potaſh; and edulcorated.

The alkaline ſolution (A *b*) was ſaturated with muriatic acid. A copious precipitation took place of a white earth, which was rediſſolved by a further addition of acid. The contents of this ſolution were thrown down by a ſolution of carbonat of potaſh. This precipitate collected on a filtre, was waſhed with diſtilled water, till it paſſed through it taſteleſs.

Dry precip. treated with ſulph. acid left ſilex.

It was dried and introduced into a matraſs, and rectified ſulphuric acid, diluted with diſtilled water, dropped into it, and the fluid was evaporated. Freſh portions of acid were abſtracted from it, as long as it ſeemed to act upon it. The ſoluble part was extracted by diſtilled water, and the reſiduum was edulcorated with diſtilled water, till it ceaſed to produce any change upon nitrat of mercury; dried, and heated red hot for half an hour, it weighed 10 grains. It was pure ſilex.

This ſulph. ſolution with acet. of potaſh gave alum.

The ſolution which the ſulphuric acid had effected, mixed with the water with which the reſiduum had been edulcorated, was gradually evaporated, and a ſufficient quantity of acetite of potaſh was added to it. Regular cryſtals of alum were formed to the laſt. A ſmall portion of ſilex, amounting to about ½ a grain, was ſeparated.

The alum. was decomp. by carb. ammon.

Diſtilled water was poured upon the alum, in a quantity ſufficient to diſſolve it in a warm ſtate; and whilſt warm, the ſolution was decompoſed by a ſolution of carbonat of ammonia. The ſpungy earth which was ſeparated, was collected on a filtre and edulcorated with diſtilled water, till it produced no effect upon nitrat of mercury.

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* The pruſſiat of potaſh which I employed, was prepared by dropping a ſolution of tartrite of potaſh into a ſolution of pruſſiat of lime, as long as any tartrite of lime is precipitated. Large and beautiful cryſtals of pruſſiat of potaſh are produced by evaporating the fluid, which I have found to remain unaltered for ſeveral years. By this method the intruſion of the ſulphuric acid is prevented: but Mr. Henry's late ingenious proceſs ſeems to be, for many reaſons, preferable to it. The pruſſiat of titanium promiſes I think, to be uſeful in the arts. Painters in oil and water colours would find in it a valuable acquiſition, as a beautiful tranſparent green.

Precip. of earth freed from alk. by acet. acid and heat.

The alumina was dried gradually, and heated in a crucible, ſo that the bottom of it was only of a low red : it weighed 111$\frac{5}{8}$ gr. Expoſed to a ſtrong red heat for half an hour, it=87$\frac{3}{4}$. Suſpecting that a ſmall portion of alkali was retained by it, I rubbed it to a fine powder, and digeſted it with acetic acid, for many hours, and ſaturated it with ammonia.

The alumina ſufficiently edulcorated, dried and expoſed to a ſtrong red heat for half an hour, now weighed 81$\frac{3}{4}$. 100 grains of the original pounded ſpar, from which I had taken the 100 grains on which I operated, were expoſed to the ſame heat, and for the ſame time, and loſt $\frac{15}{16}$ of a grain, as I had found before.

Component parts.

The proportion which the ſeveral ingredients of this ſtone bear to each other, according to the foregoing analyſis, will be,

Alumina - - - - -	[C *d*]	- - - - - - -	81,75
Silex - - - - - -	[B *b*] [C *b*] [C *c*]	1,625 10 0,50	12,125
Oxide of titanium -	[B *d*]	- - - - - - -	4
Water - - - - -	[A *a*]	- - - - - - -	0,937
Loſs - - - - - - -		- - - - - - - -	1,188
			100

Analyſis repeated.

I have repeated the analyſis of this mineral * ſeveral times, and have always found it to contain the ſame ingredients; though there has been a ſlight variation as to the relative quantities of each. This we might expect to be the caſe, not only from the loſs inſeparable from the operations to which the ſtone is ſubjected, but alſo from the nature of the ſtone itſelf, as it is not perfectly homogeneous. In one inſtance, I found the alumina amount to 84 per cent. and the ſilex proportionably diminiſhed.—Until we have better means than we have at preſent, of aſcertaining the real quantities of water and oxygen which the earths and the oxides of metals ſeverally retain in the natural ſtate in which they exiſt, as ingredients of complex ſtones, we muſt conſider the reſult of chemical analyſis, but as an approximation towards the ſtrictneſs of truth.

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* I am indebted to the kindneſs of Mr. Raſhleigh, for another ſpecimen of corundum from the ſame place, which differs in ſome reſpects in its external appearance from that which I examined. Its colour is not of ſo bright a lilac, and it ſeems of a more compact and uniform grain. Its ſurface is covered with a coat of yellow mica.—This I have not ſubjected to experiment.

As the intruſion of the oxide of titanium into the place, which has been uſually occupied by the oxide of iron, conſtitutes this ſtone a variety of the adamantine genus, I have been induced to record my experiments. I am aware, however, that it may be juſtly obſerved, that the oxide of neither metals is eſſential to the nature of the corundum.

Oxide of titanium often met with.

I have no doubt, but that the oxide of titanium will be found to be more plentifully ſcattered abroad throughout the mineral kingdom, than it has been hitherto imagined. I have detected it in a ſpecies of ſhirl, which occurs in a large cryſtallized maſs, on the tenement of Botallack, in the pariſh of Juſt, in this county. It compoſes ſeveral alternate beds or floors of varying dimenſions.—The compactneſs of theſe beds not having allowed the cryſtals to ſhoot with freedom in any direction; It is rarely that they occur with their terminations compleat.—Theſe, however, in no reſpect differ from the common form, and vary only in the length of the priſm, from $\frac{1}{4}$ of an inch to an inch.—The colour of this ſhirl is black.—I have alſo lately diſcovered the oxide of titanium in two ſpecies of baſalt, of the form of large pebbles, which I found near the ſea-ſhore. Whether they are natives, or whether they were dropped upon our coaſt accidentally as ballaſt from ſome veſſel, I have not as yet had an opportunity of aſcertaining. That they are *real baſalts*, I reſt upon the authority of one on whom I can ſafely rely, whoſe knowledge in mineralogy can be only equalled by his friendly diſpoſition to communicate it. I mean John Hawkins, Eſq.

My experiments alſo proved, that they contained all the known ingredients of baſalts, with the addition of the oxide of titanium.

Creed, near Grampound, Cornwall,
March 2, 1803.

Letter

II.

Letter from Mr. WILLIAM HENRY. *Concerning the Invention of Aromatic Vinegar.*

To Mr. NICHOLSON.

SIR,

The appropriation of diſcoveries is matter of general concern.

I AM ſenſible that an apology is neceſſary for intruding on you and on your readers, a ſubject which may appear, on firſt view, to have little claim to general attention. If the rights and intereſts of an individual were alone involved on this occaſion, I ſhould not have requeſted a place in your Journal, for the following ſtatement. But it is ſurely matter of general concern, that the appropriation of diſcoveries and improvements ſhould be dealt with ſtrict juſtice to their authors: for the proſpect of this diſtribution of " honour where it is due," is one of the moſt animating principles of action; and the extinction of this motive would follow an indifference on the part of the public, to the claims of inventors.

Diſcovery that acetic acid diſſolves camphor and eſſential oils.

More than fifteen years ago, during the delivery of a courſe of chemical lectures by my father, in this town, he had occaſion to notice a quality of the acetic acid or radical vinegar, which had not to his knowledge been obſerved before; viz. its property of diſſolving camphor and various eſſential oils. The compound was found to poſſeſs a moſt pungent and agreeable odour; and as the *vinaigre des quatres voleurs* had gained much reputation in preventing infection, it occurred to him that the newly diſcovered ſolution would have ſtill more powerful effects, in conſequence of its high ſtate of concentration. A bottle of this preparation he gave to a late active magiſtrate and philanthropiſt (T. B. Bayley, eſq. F. R. S.) who, in the courſe of an unwearied and undaunted exerciſe of his public functions, was frequently expoſed to the dangers of foul and infected air. Mr. Bayley was highly gratified by its effects; and not only made conſtant uſe of the aromatic vinegar on the bench, and on his viſits to the priſon, but introduced it to the adoption of ſeveral of the judges, and principal gentlemen at the bar. He alſo firſt ſuggeſted to my father the propriety of benefiting by his diſcovery, and was the medium of a connection with Mr. Bayley, perfumer, in Cockſpur Street, which has been continued to the preſent day.

The ſanction of an eminent phyſician given to a ſubſequent claimant.

The aromatic vinegar, like all other articles in general demand, has been a frequent ſubject of imitation. But it is not of this that I complain; for in conſequence of unremitting attention, our preparation has maintained a decided ſuperiority over all others, both as to quality and extent of ſale. The occaſion of this appeal to your readers is, that one of theſe imitations has been ſanctioned by the name of a reſpectable phyſician, who, though not expreſsly yet by implication, confers on another the credit of that invention, which in juſtice is due to my father. *(See a letter from Dr. Trotter, phyſician to his majeſty's fleet, contained in the advertiſement of a London druggiſt.)*

Subſequent proceedings, &c.

From the recommendatory letter alluded to, it is ſufficiently evident that Dr. Trotter was unacquainted with any prior claim to the invention of the aromatic vinegar; and he was therefore furniſhed by my father, in the moſt reſpectful manner, with the facts that have been laid before you. To this letter the doctor has never replied, though he declared verbally, to a medical gentleman, that my father's preparation had never fallen in his way; and that if it had, he ſhould with equal readineſs, have given teſtimony in its favour. The advertiſement, however, ſtill continues to be regularly inſerted; and I therefore deem it expedient to appeal thus publicly, againſt the injuſtice of ſuch a proceeding; eſpecially in behalf of a man, who has imitated the original, only in aſſuming, with the cooleſt effrontery, an advertiſement drawn up by myſelf.

I believe there are few of your readers, who will not decide, that the ordinary forms of civility required Dr. Trotter to have taken ſome notice of the letter that was addreſſed to him; that ſuch an attention ought to have been paid to one of the oldeſt practitioners of medicine in this country; and that more reſpect was due to a man (whom I truſt it is not unbecoming me to characterize, in terms already publicly applied to him, *viris laudatis* *) "reſpectable in ſcience and literature," and "diſtinguiſhed by ingenuity, honour, and the ſtricteſt integrity."

I am, Sir,
your obliged friend and ſervant,
WILLIAM HENRY.

Mancheſter, March 13, 1803.

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* Dr. Aikin and Dr. Percival.

III

Caution againſt the Danger of leaving Phoſphoric Preparations in the Vicinity of Wood. By a Correſpondent.

To Mr. NICHOLSON.

March 1, 1803.

SIR,

ACCIDENTS in chemical experiments not ſeldom afford uſeful facts or ſuggeſtions; but I am doubtful whether or no the following occurrence will be thought worth notice in your Journal: however, the account of it is at your ſervice.

Narrative of fire occaſioned by oxidule of phoſphorus.

While at lecture this morning, we were ſuddenly annoyed by a column of white fumes iſſuing from amongſt bottles of preparations on a ſhelf, which was ſoon followed by flame. On examination I found the inflammation proceeded from a bottle containing lime, into which phoſphorus had ſublimed in the upper part of a tube in the making of phoſphuret of lime. This was not therefore phoſphuret of lime, but only the mixture of particles of ignited phoſphorus, with pulverized lime ſcarcely ignited. In this circumſtance the phoſphorus I know becomes oxigenized to be in the ſtate of *oxidule*, if not of *oxide;* which compoſition it is well known, I ſuppoſe, is employed for the charging of little bottles to inflame ſulphur matches by mere friction of them within ſide of the bottle. In the preſent caſe, the bottle containing the lime with phoſphorus had cracked, ſo as to admit air, which excited the inflammation at the temperature of about 65°. Such preparations ſhould therefore be kept out of the way of wood, or any inflammable ſubſtance, for if this accident had happened when no one was preſent, the whole laboratory would have been ſet on fire.

Your's truly,

Z.

IV. *Deſcription*

IV.

Deſcription of a new Reflecting Quadrant. By Mr. EZEKIEL WALKER. *From the Author.*

To Mr. NICHOLSON.

SIR,

Obſervations on Hadley's quadrant.

THAT inſtrument which goes by the name of Hadley's Quadrant, may perhaps be deemed one of the moſt uſeful inventions of the laſt century. Although its arc is only the one eighth part of a circle, yet it is ſo conſtructed as to meaſure angles from 0° to 90° by the fore obſervation, and from 90° to 180° by the back obſervation. But as no method has yet been found out, by which the back horizon glaſs can be adjuſted with the ſame exactneſs as the fore one, all angles above 90° which are taken by the back obſervation, cannot be relied on ſo much as thoſe that are taken by the fore obſervation. To obviate this inconvenience, the ſextant was invented, by which any angle leſs than 120° may be taken by the fore obſervation. This is an invaluable inſtrument to the nautical aſtronomer; and indeed it is much to be regretted, that its uſe is not ſo generally underſtood by travellers, as the imperfect ſtate of geography requires. It muſt, however, be admitted, that it is not quite free from imperfections. Firſt, an angle greater than 120° cannot be taken by it; and ſecondly, when a large angle is taken, the rays of light fall ſo obliquely upon the index glaſs as may occaſion ſome doubt reſpecting the truth of the obſervation. Mr. Ludlam gives it as a general rule, in conſtructing an octant, "that very oblique reflections from the mirrors ought to be avoided *." If it be neceſſary to obſerve this rule in conſtructing an octant, the ſame ſhould be attended to, as much as poſſible, in taking obſervations.

Imperfections of the ſextant.

New inſtrument meaſuring to 180° without oblique reflection.

Theſe imperfections ſuggeſted the idea of an inſtrument to meaſure any angle between 0° and 180° by the fore obſervation, without very oblique reflections from the mirrors. Plate XII. Fig. 2. exhibits a plan of this inſtrument, where L M C repreſents an octant, conſtructed in the uſual way, A B the index glaſs, m the horizon glaſs, and H E the line of ſight, or axis of the teleſcope. Upon this line of ſight, at an angle of 45 degrees, let another horizon glaſs be fixed, repreſented by $x\,y$, which may be called *the ſecond horizon glaſs.*

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* Ludlam on the uſe of Hadley's quadrant, par. 91.

When a ray of light R n falls upon the centre of the glaſs $x\,y$, at an angle of 45 degrees, it will be reflected to the eye at E; becauſe the angle $x\,n$ E (the angle of reflection) is = 45° by conſtruction, and conſequently R n H and R n E are right angles.

The Uſe of the Reflecting Quadrant in taking Angles.

Manner of uſing it.

All angles leſs than 90° are to be taken by this inſtrument, in the ſame manner as by the ſextant. But, ſuppoſe it were required to obſerve the ſupplement of the ſun's meridian altitude at ſea, let the axis of viſion be directed to the zenith, and turn the quadrant till that part of the horizon, which is oppoſite to the ſun, be ſeen by reflection from the ſecond horizon glaſs; then turn the index from 0 towards M, till the ſun appears in the teleſcope, with one of its limbs in contact with the horizon, and the index will ſhew the ſun's obſerved zenith diſtance; to which add 90°, and the ſum will be = the angle obſerved by the quadrant, = the obſerved ſupplement of the ſun's meridian altitude.

To adjuſt the Second Horizon Glaſs.

Adjuſtment.

Let the arc L N be made equal to the arc L M, and graduated from 0° at L, to 90° at N. Then, after the index error has been determined, ſet the index to 90° at N, and elevate the teleſcope till the ſun, or ſome other remote object, be ſeen by reflection from the index and horizon glaſſes, and the ſame object will alſo be ſeen by reflection from the ſecond horizon glaſs: conſequently, the angle R n H becomes known to as great a degree of preciſion as the index error.

I am,
SIR,
Your obedient ſervant,
EZEKIEL WALKER.

Lynn Regis, Feb. 20, 1803.

V. *Miſcellaneous*

V.

Miſcellaneous Information. Miſtake reſpecting Dr. Thomſon, Author of the Elements of Chemiſtry. Obſervations on the Scottiſh Querns. On the ſuppoſed Determination of the real Zero of Heat. In a Letter from a Correſpondent.

To Mr. NICHOLSON.

SIR,

Miſtake reſpecting Dr. Thomſon.

I BEG leave to obſerve a ſmall inaccuracy in the notice * of Dr. Thomſon's Syſtem of Chemiſtry. Thomas Thomſon, M. D. Lecturer on Chemiſtry in Edinburgh, who is much eſteemed for his extenſive knowledge, is the author of that excellent work. The Notes on Fourcroy's Elements of Chemiſtry were written by Mr. John Thomſon, ſurgeon in Edinburgh.

The following remarks are at your ſervice, from

SIR,

Your very humble ſervant,

H.

Edinburgh, Feb. 17. 1803.

1. *Obſervations on the Scottiſh Querns.*

The Scottiſh quern or hand mill.

The Indian hand mill, deſcribed in the Philoſophical Journal (III. 186), very much reſembles the hand mill or *querns* uſed by the cottagers in Scotland for grinding malt. There are many varieties of querns, but the greater number of them anſwer to the deſcription juſt quoted, to which I beg leave to refer the reader.

The querns in Aberdeenſhire have a peculiar form: a cylindrical cavity in the under ſtone contains the upper one. A plug of wood is driven in the center of this ſtone for fixing the gudgeon, and there is an eye or aperture in the angle through which the ground malt runs out. Inſtead of cutting ſo much out of the ſolid ſtone, it would be better to uſe a wooden drum. It is unneceſſary for me to give a more minute detail of the Scottiſh querns, which have been long known in this country; ſeveral pieces of them having been found in the ruins of very old buildings.

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* Phil. Jour. 8vo. III. 62.

Mill of Dumfriesſhire.

In Dumfriesſhire there is a ſmall pair of mill ſtones on a frame, driven by a crank, a vertical cog wheel, and a horizontal pinion, which grind very well, and are extremely uſeful. The ſtones have a drum and hopper like a ſingle mill. An eaſy expeditious method of reducing grain to flour, is of the utmoſt conſequence to the inhabitants of every civilized country, and ingenious men deſerve our beſt thanks for their reiterated attempts to obtain ſo deſireable an end.

II. *Obſervations on the ſuppoſed Determination of the real Zero.*

On the zero of heat.

Many eminent chemiſts have endeavoured to diſcover the real zero, or the diviſion on the thermometric ſcale correſponding to the total privation of caloric, but their labours have hitherto been unſucceſsful. Dr. Irvine of Glaſgow contrived a theorem for this purpoſe, which he founded on ſuppoſitions and experiments; and Mr. Dalton has lately propoſed an hypotheſis for the ſolution of the ſame problem. Theſe may be conſidered in their order.

Dr. Irvine.

Mr. Dalton.

Without giving Dr. Irvine's theorem, which is ſo well known, I ſhall inſert the principal reſults of the experiments and calculations founded on it.

Determinations of zero from Irvine's theorem.

In the following tabular view, the numbers in the firſt column denote degrees of Reaumur's ſcale *above* or *below* the freezing point, and thoſe in the ſecond column indicate degrees of Fahrenheit's ſcale *above* or *below* the ſame point, accordingly as the ſign *plus* or *minus* is prefixed reſpectively.

	Reaumur.	Fahrenheit.
M M. Lavoiſier and Laplace, from experiments on a mixture of nine parts of water and 16 parts of quick lime, place the real zero at - -	—1537°	=—3458¼°
Their experiments on a mixture of ſulphuric acid and water, in the proportion of 4 to 3 reſpectively, fix it at	—3241	=—7292¼
Their experiments on a mixture of the ſame fluids in the proportion of 4 to 5 place it at - - - -	—1169	=—2630¼

And

	Reaumur.	Fahrenheit.
And the experiments of theſe celebrated philoſophers, on a mixture of nitrous acid and quick lime in the ratio of $9\frac{1}{3}$ to 1, give for the real zero, $\frac{1889}{-0.01783}$ - - - -	$+10594\frac{1}{2}$	$+23837\frac{3}{5}$
Seguin, from the experiments of Lavoiſier on the combuſtion of phoſphorus narrated in his Elements of Chemiſtry, determines the place of the real zero to be - - - - -	-842	$-1894\frac{1}{2}$
He found, by the experiments of Lavoiſier on the combuſtion of carbon, that it ſhould be - - -	-1204	-2709
And he inferred, from Lavoiſier's experiments on the combuſtion of hidrogen gas, that the real zero ſhould be at - - - - -	-739	$-1662\frac{1}{4}$
The compariſon of the capacities of water and ice, which, according to the experiments of Kirwan, are as 1 to 0.9, fix the real zero at * - -	-600	-1350
Dr. Crawford, from experiments on the combuſtion of hidrogen gas, places the real zero at † - - -	$-680\frac{8}{9}$	-1532
Gadolin's experiments on the converſion of ſnow into water, taking the capacity of ſnow to that of water as 9 to 10 according to Magellan, place the real zero at ‡ - - - -	$-649\frac{3}{5}$	$-1461\frac{3}{5}$

Theſe reſults differ widely, and one is impoſſible.

Theſe ten widely different reſults, of which one is impoſſible, induce us to conclude that one of Dr. Irvine's ſuppoſitions at leaſt is erroneous. Indeed, it ſeems to be agreed that his theorem is not well founded. The ſeventh and ninth numbers alſo ſhew, that a near approximation to accuracy is

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* An intereſting Paper by M. Seguin, from which the firſt eight numbers are taken, may be ſeen in the Annales de Chimie, V. 255.

† Crawford on Animal Heat, 267.

‡ Ibid. 458.

ſcarcely attainable in theſe experiments. Let us now conſider the ſecond hypotheſis.

Mr. Dalton's hypotheſis.

In the Philoſophical Journal, Vol. III. page 130, there is a very important paper of Mr. Dalton's on the Expanſion of Elaſtic Fluids, where he ſays, "In order to explain the manner in which elaſtic fluids expand by heat, let us aſſume an hypotheſis, that the repulſive force of each particle is exactly proportional to the whole quantity of heat combined with it, or in other words, to its temperature reckoned from the point of total privation: then, ſince the diameter of each particle's ſphere of influence is as the cube root of the ſpace occupied by the maſs, we ſhall have $\sqrt[3]{1000} : \sqrt[3]{1325}$ (10 : 11 nearly) : : the abſolute quantity of heat in air of 55° : the abſolute quantity in air of 212°. This gives the point of total privation of heat, or the abſolute cold, at 1547° below the point at which water freezes."

Computations of zero made from the ſame.

According to this hypotheſis, I have computed the following numbers, where the ſame is to be underſtood as in the laſt table, except the degrees in the firſt column, which are thoſe of the centigrade thermometer.

Celſius. Fahrenheit.

$\sqrt[3]{1000} : \sqrt[3]{1325} :: 10 : 10.983446,$
which places the real zero at - —874°.12=—1573°.43

$\sqrt[3]{1000} : \sqrt[3]{1375} :: 10 : 11.1199,$
which fixes it at - - - —892.93=—1607.29

$\sqrt[3]{1167} : \sqrt[3]{1325} :: 10.528 : 10.9834,$
nearly, which gives - - - —952.34=—1714.2

$\sqrt[3]{1000} : \sqrt[3]{1167} :: 10 : 10.528268,$
and the real zero is at - - —827.77=—1485.98

Theſe reſults apparently of little value.

Theſe deductions do not differ nearly ſo much as thoſe in the firſt table; yet they give grounds for believing that the ſuppoſition, or the greater part of the experiments is erroneous. It is no ſupport to the validity of this hypotheſis to ſay, that the firſt number nearly agrees with one deduced from a method totally different; for it might have been conſiderably greater or leſs than it is, and yet have been nearly equal to one of thoſe in the firſt table. There ſeems to be a ſlip in the degrees mentioned in Mr. Dalton's paper; for 167 + 158 = 325, the whole dilatation between 55° and 212°, while 77½°

$+ 77\frac{1}{2}^\circ = 155^\circ$ only, which ſhould be 157°, of which the half is $78\frac{1}{2}^\circ$. This might eaſily happen: in the calculations of the third and fourth number I have uſed $78\frac{1}{2}^\circ$.

Mr. Dalton and Cit. Gay Luſſac have conſiderably improved the method of experimenting on gaſes, and it is to be hoped they will puſh their improvements ſtill further.

H.

VI.

Outline of the Craniognomic Syſtem of Dr. Gall of Vienna. By Dr. BOJAMES.

(Concluded from Page 202.)

5. *Organ of the Inſtinct of Sexual Union,*

5. Sexual union.

THE organ of the inſtinct of ſexual union is ſituated at the baſe of the *occipital,* behind the *medulla oblongata,* and the large aperture of the *occipital.*

This organ is not developed until the age of puberty; and it in a great meaſure influences the figure of the back of the neck, becauſe the muſcles are attached to this part of the *cranium.*

The developement of this organ does not take place in animals which are caſtrated before puberty, for which reaſon it is invariably obſerved that bulls have much ſtronger necks than oxen, and " that horſes which have undergone this operation, before the neck is formed, are always ſlender in that part."

In the monkey, the hare and the cock, this organ is very diſtinct, and in pigeons and ſparrows the *occiput* forms a peculiar ſack, which appears to be an appendage to the head; it is alſo known that theſe animals have a ſtrong diſpoſition to copulate. The ſame configuration is ſometimes found in the cranium of men, and Gall poſſeſſes in his cabinet ſeveral ſkulls of idiots, who were noted for laſciviouſneſs, in which the *occiput* has a very unuſual projection.

6. *Organ of the Mutual Love of Parents and Children.*

6. Parental and filial affection.

The organ of the mutual love of parents and children occupies all the back and upper part of the *occipital;* from its poſition

tion it is intimately connected with the preceding organ, which conſequently influences this by its action. "The exceſſive developement of this organ ſometimes contributes to form that prolongation of the *occiput* of which we ſpoke in the preceding article."

This organ is in general more diſtinct in women than in men, and throughout nature it is more defined in the female than the male; it is more particularly apparent in monkeys, whoſe fondneſs for their young is ſo remarkable as to have become proverbial.

"In general, all thoſe animals which ſhew much affection for their young are provided with it; and it appears to us that pigeons, the male of which ſits on the eggs as well as the female, and which feed their young neſtlings by a kind of rumination, may be taken as an example."

The cuckow, which never rears its young, is almoſt entirely deſtitute of this organ.

7. *Organ of Attachment, of Friendſhip.*

7. Attachment; friendſhip.

Behind and between the parietals, and on the lateral parts of the *occipital*, is placed the organ of attachment, or of friendſhip.

"By its poſition it has an intimate connection with the two preceding organs, and it is in animals deſtined to live in ſocieties that the united action of theſe three organs takes place."

Dogs ſhew the moſt aſtoniſhing marks of attachment, and ſpaniels, terriers, and houſe-dogs afford the greateſt number of examples; theſe ſpecies are alſo diſtinguiſhed by a large head, in which the developement of this organ is found behind and above the *zygomatic apophyſes*. The head of the greyhound, which is leſs ſuſceptible of attachment, is narrower behind, and uſually without this organ.

8. *Organ of Courage.*

8. Courage.

The organ of courage is placed at the poſterior and inferior angle of the parietals. It aſſiſts in increaſing the ſize of the head and ſeparating the ears from each other. Its proximity to the three preceding organs accounts for the fury of animals in rutting time, and for the extraordinary courage of thoſe which have young, or which protect their females or the individuals of their ſociety.

It is moſt diſtinct in the hyena, the lion, the wolf, ſome ſpecies of dogs, and particularly in the wild boar, the fierceneſs of which is well known.

On the contrary, the aſs, the greyhound, the ſheep, the hare, which are diſtinguiſhed by their timidity, are deſtitute of this organ; their heads are narrow behind, and their ears almoſt cloſe together.

A ſingular phenomenon ſeems to give countenance to the opinion of Gall reſpecting the ſituation of this organ; it is the involuntary action of a man who loſes his courage. He rubs behind his ears, as if endeavouring to ſtimulate the action of that organ from which this faculty is derived.

Note, " We have noticed an action of cats which appears to have ſome reſemblance to the above, and which regards the organ of attachment. It is, that in careſſing man they rub againſt him with the back part of the head."

9. *Organ of the Inſtinct to Aſſaſſinate.*

9. Aſſaſſination. The organ of the inſtinct to aſſaſſination is ſituated more forward than the organ of courage, towards the middle of the ſides of the parietals.

It is developed in all the carnivorous animals who live by prey; and Gall found it in the ſkulls of ſeveral murderers.

10. *Unknown Organs.*

10. Unknown. Two organs which correſpond with the temporal bones, are yet unknown as to their functions.

11. *Organ of Cunning.*

11. Cunning. The organ of cunning occupies the front and lower parts of the parietals; it is developed in all thoſe animals which are diſtinguiſhed for this faculty, ſuch are the fox, the pole-cat, the cat, the diver[*], and it is in the moſt intimate combination with the *organ of theft,* which is only a prolongation of this towards the orbit, and is found in cats, ſome dogs, and in magpies.

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[*] An obſervation which it appears difficult to reconcile to this, is that Gall conſtantly obſerved this organ to be developed in poets; he gives no explanation, but his obſervation is accurate.

It is perhaps to the developement of this organ that the Calmucks, whoſe national character is that of theft, are indebted for that magnitude of their heads which has been noticed by ſome obſervers.

12. *Organ of Circumſpection,*

The organ of circumſpection is ſituated in the middle of the parietals, above the organ of cunning, and that of the inſtinct to aſſaſſination. **12. Circumſpection.**

The exceſſive developement of this produces irreſolution, and its defect cauſes heedleſſneſs; it is diſtinct in the chamois and the rein-deer, the circumſpection of which is remarkable, and which do not tread over unknown paths but with the greateſt precaution.

It is alſo found in ſuch animals as only leave their retreats at night, ſuch as owls, otters, &c.

13. *Organ of the Inſtinct for Self-Elevation.*

This organ, in the middle of the inner border of the parietals, a little farther back than the middle of the upper part of the head, gives us a true notion of the difficulties to be overcome in the reſearches of Gall, and at the ſame time affords a ſtriking example of the happy opinions of this accurate obſerver. **13. Self-elevation.**

He found this organ well developed in the chamois, and ſtill more ſo in the wild goat; he alſo noticed the ſame in many men diſtinguiſhed by their pride. It was difficult to bring theſe obſervations into one point of view; but on conſidering that the chamois frequents the moſt lofty parts of mountains, that the wild goat is conſtantly endeavouring to aſcend higher, and that pride, attentively examined, is only the deſire to be ſuperior to others, he was perſuaded that this was the organ which produced theſe effects, *apparently ſo differing,* and he took it for the organ of the inſtinct of raiſing or ſelf-elevation.

The head of the proud man, raiſed and thrown back, tends to confirm this opinion ſtill more,

Note, " It appears to us that the figure of a proud man, oppoſed to that of a ſubmiſſive and modeſt man, renders the truth of this notion more obvious. In the firſt every thing is directed upwards; he ſets up his hair, raiſes his head, lifts his brows, turns up his eyes, throws back his ſhoulders, walks on tip-toe, and conſiders every ſurrounding object as beneath him; in the other, on the contrary, the hair falls naturally, the eyes, the eye-lids and the head are turned downwards, the body and the knees are ſlightly bent; in ſhort, every thing indicates a ſtate of ſubmiſſion, without a wiſh for ſuperiority."

14. *Organ of the Love of Glory.*

14. Love of glory.

When this organ is extended farther on the ſides, it forms that of the love of glory, a propenſity very analogous to pride.

15. *Organ of the Love of Truth.*

15. Love of truth.

The function of the organ which is ſeen at the poſterior and ſuperior angle of the parietals, is not exactly fixed by Gall; nevertheleſs he has reaſons to conſider this angle as the ſeat of the organ of the love of truth; but he has not yet collected a ſufficient number of facts to produce entire conviction.

Note, "We have ſome difficulty to perſuade ourſelves of the function attributed by Gall to this laſt organ; it appears to us, that an organ found among thoſe with which animals are provided as well as men, ought not to be appropriated to a faculty, like the attribute of veracity, is adapted only to the latter.

"Nevertheleſs this faculty, like that of pride, may be capable of great modifications in animals: and we acknowledge that we have known two men, one of whom was diſtinguiſhed by extreme veracity, and was furniſhed with this organ in a very eminent degree; but the other, on the contrary, whoſe diſpoſition for falſhood was extraordinary, was ſo entirely deſtitute of it, that inſtead of a projection, there was a cavity in this part of the head."

In the anterior, or lower part of the *os frontis,* Gall diſcovered many organs, the functions of which are of great importance.

At the commencement of his inveſtigations, he conſidered them as organs of the different ſpecies of memory; but afterwards obſerving that their action was not merely reproductive, but alſo productive, he was induced to conſider them as the organs of particular ſenſes, and to eſtabliſh, from this obſervation, the opinion that memory in general is only the reproductive operation of all the organs; and that imagination, on the contrary, is their productive action.

The automatic motion of man who endeavours to recollect, ſeems to relate to theſe organs. He places his hand, unconſciouſly, upon the lower part of his forehead. This action, though unobſerved by him who performs it, is nevertheleſs conſtant, and is never confounded with that mentioned before, under the head of the organ of courage.

16. *Organ of the Senſe of Locality.*

16. Senſe of locality.

The organ of the ſenſe of locality occupies the fore part of the *os frontis*, which correſponds to the protuberances above the orbits *(protuberantiæ ſuprà orbitales)*; it is generally found in the craniums of thoſe who are diſtinguiſhed by large frontal ſinuſes, and uniformly have an anterior cavity adapted to an elevation of the brain.

When it acts reproductively, it conſtitutes what we term local memory *(memoria localis)*; by its productive operation, on the contrary, it inclines to combinations of new localities.

It is this organ which, in unknown places, directs the blood-hound, in whom it is ſtrongly defined; it exiſts in all birds of paſſage; it incites them to change of place, to take long flights, and to return to their firſt habitations: the ſtork and the ſwallow are eminently provided with it, as well as thoſe animals which remove the fartheſt from our climates. In ſuch men as are furniſhed with it, we alſo diſcover a ſtrong remembrance of places and a deſire to travel; it is alſo conſtantly found in good painters of landſcapes.

" A general, who regulates the diſpoſitions of an army, and who at a glance muſt diſcern all the localities of the country he occupies, cannot ſucceed without it." The great Frederic furniſhes us with a ſtriking example. In advanced age, this organ is one of thoſe which ſenſibly diminiſhes: it is alſo notorious that every kind of memory and of imagination grow weaker as the individual becomes older; the frontal ſinuſes then increaſe within; the action of the brain is no longer capable of obſtructing their developement.

17. *Organ of the Senſe of Things* (ſenſum rerum.)

17. Senſe of things

The ſenſe of things has its correſpondent organ in the lower and anterior part of the *os frontis*, between and higher than the preceding; its action is both productive and reproductive; and, in the latter caſe, it gives the remembrance of facts and things.

This organ is very neceſſary to education and inſtruction, which abſolutely require the recollection of paſt circumſtances; in age it is ſubject to the ſame change as the preceding.

Among animals, the elephant is particularly diſtinguiſhed by the developement of this organ; this animal alſo retains with the greateſt accuracy thoſe circumſtances and acts which have a reference to itſelf.

"Among men, we have found this organ, not only in thoſe whoſe memory of facts and objects was powerful, but alſo in thoſe which might be denominated ſyſtematic heads, from arranging facts in order, and hence forming concluſions; in thoſe who poſſeſs a happy conception, and are diſtinguiſhed by a deſire of univerſal knowledge; it even appears to us, that the operation of combining facts with a view to obtain a reſult, is one of the principal actions of this organ; at leaſt the elephant, who keeps his trunk filled with water, for the purpoſe of throwing it over the man who had offended him the night before, arranges a number of facts, and draws from them a reſult which is a true logical concluſion; and we are not acquainted with any other organ in the elephant to which this action can be referred.

"The automatic movement of the man who perceives that he has reaſoned wrong ſeems to give effect to theſe conjectures. He ſtrikes himſelf on the middle of the forehead."

18. *Organ of Painting, Senſe of Colours.*

18. Painting or colours.

The organ of the ſenſe of colours, or of painting, occupies the fore part of the *os frontis*, above the orbit; Gall has obſerved this organ in all painters of eminent talent.

"Having been acquainted with this diſcovery but a ſhort time, we have not been able to collect many obſervations on it; we have neverthelefs remarked it in ſome individuals, and it is very apparent in the head of Raphael, in the National Muſeum, No. 57."

19. *Organ of the Senſe of Numbers.*

19. Senſe of numbers.

The organ which correſponds to the lower and exterior part of the *os frontis*, near the zygomatic apophyſis of that bone, poſſeſſes the function of the ſenſe of numbers; it exiſts in men whoſe memory is good with reſpect to numbers, and in arithmeticians who combine calculations with facility; it exiſts in a ſpecies of magpie capable of counting as far as nine, which is the only example known among animals.

" We have had an opportunity of noticing this organ on the head of a blind man, at the Quinze-Vingts, remarkable for his arithmetical talents; and Gall preſerves the buſts of many men which afford very inſtructive examples."

20. *Organ of the Muſical Senſe.*

20. Muſic.

Above this organ is found that of muſical ſenſe, or for ſounds.

It acts in a manner ſimilar to the other organs, productively and reproductively; it gives the memory for ſounds; it facilitates the new combinations of muſical compoſition; it induces birds to ſing; it acts upon thoſe who learn to ſpeak, and in whom language is founded only upon this remembrance of ſounds.

It is intirely wanting in animals which have no muſical ſenſe; it is ſtrongly developed in the parrot and the ſtarling; and thoſe great muſicians Gluck, Mozart, Haydn, Pleyel, furniſh us with ſtriking examples.

21. *Organ of Senſe for Mechanics.*

21. Mechanics.

In the lateral and inferior part of the *os frontis* is found the organ of ſenſe for mechanics. The beaver which forms its habitation is eminently provided with it; it exiſts in the field-mouſe and in the birds which make their neſts with much art; it is met with in men of mechanical talents, who conſtruct with eaſe any machine, who uſe their hands with dexterity, and who excel in the different arts which require manual labour. Though it may be difficult to judge of the exiſtence of this organ, when it is but ſlightly developed, " becauſe the temporo-maxillary muſcle covers this part of the cranium; it is neverthele ſs very apparent if the faculty exiſt in a ſuperior degree, and it is then one of thoſe organs reſpecting which there can be the leaſt doubt.

22. *Organ of Verbal Memory.*

22. Verbal memory.

In the interior of the orbit, at the bottom of the upper part, exiſts the organ of verbal memory; it may be noticed from its developement by the effect it produces on the poſition of the ball of the eye, which it impels forwards, and more or leſs out of the orbit.

Perſons provided with it eaſily retain words by heart. Gall, while young, remarked this faculty in ſeveral of his ſchool-fellows, who excelled merely by this talent, and who were diſtinguiſhed by the protuberance of their eyes. This was the firſt obſervation which afterwards led him to theſe inveſtigations. A number of ſubſequent obſervations have eſtabliſhed the truth of its exiſtence and of its function.

23. *Organ of Senſe for Languages.*

23. Languages. The organ at the exterior and upper part of the orbit, is called by Gall, the organ of ſenſe for languages. Its preſence has a conſiderable influence upon the poſition of the ball of the eye; it puſhes it downwards and towards the noſe, and increaſes its diſtance from the upper edge of the orbit; it never exiſts in animals, in whom the ball of the eye is directed more towards the exterior ſide of the orbit.

Diſtinguiſhed talents for the languages are invariably attended by its developement; it is eminent in great philologiſts; and though it may be difficult to decide from external appearances, reſpecting its exiſtence, we have nevertheleſs remarked that it never eſcaped the penetrating eye of Gall, and that he was never miſtaken in this point.

24. *Organ of the Memory of Perſons.*

24. Memory of perſons. The function of the organ at the upper internal part of the orbit, has not yet been diſcovered by Gall; nevertheleſs many obſervations on man and animals, ſuch as the dog and the horſe, induced him to ſuppoſe it the organ of recollection of perſons. The developement of it, like that of the preceding, muſt influence the poſition of the eye; it ſhould contribute to remove it from the upper edge of the orbit, and to puſh it towards the external ſide, if an equal developement of the preceding organ do not counterbalance its action.

25. *Organ of Liberality.*

25. Liberality. The organ of liberality is ſituated in the anterior part of the *os frontis*, above that of the ſenſe of locality, and of the ſenſe for painting (Nos. 16 and 18), and beſide that of the muſical ſenſe (No. 20); its extreme developement is a concomitant of prodigality; it it not to be found in the miſer; in that caſe, a hollow is formed in this part of the *os frontis*. Gall is in poſſeſſion of numerous examples.

"The proximity of the organ of muſic, and that of the ſenſe for painting (Nos. 18 and 21) appear often to aſſiſt the developement of that of liberality; this is perhaps one reaſon why we ſo often find prodigality among men of eminent abilities in theſe ſciences."

We conſtantly obſerve, that as a man becomes old, he becomes covetous; thus in advanced age the diminution of this organ is ſo remarkable, that its place is frequently occupied by a cavity in the *os frontis*, which is ſometimes of conſiderable ſize.

26. *Organ of the Power of comparing Things.*

26. Comparing or combining.

The organ above the ſenſe of things, in the middle of the forehead, is deſtined to a faculty which Gall calls the ſpirit of compariſon (*judicium comparativum*).

It forms an oblong eminence, and is found in men who avail themſelves eaſily of figures and images in converſation; who are not at a loſs for expreſſions; who narrate fluently; and poſſeſs great eloquence.

27. *Organ of Metaphyſical Talent.*

27. Metaphyſics.

When this organ is more developed towards the ſides, ſo as to form a round prominence, raiſed in the middle of the forehead, it is the index of metaphyſical talent. Among the buſts of ancient philoſophers, that of Socrates affords the moſt ſtriking example; among modern philoſophers remarked for this organ, I ſhall only notice Kant as one of the moſt celebrated.

Note, "I recollect in one of my firſt ſchool-fellows, to whom we had given the name of philoſopher on account of his attachment to the abſtract ſciences; that his forehead preſented a very viſible developement of this organ."

28. *Organ of the Talent for Obſervation.*

28. Obſervation.

The organ of the talent for obſervation ſpreads over the whole of the anterior part of the *os frontis*, and its developement approaches more or leſs to the front of the vertical line. It is found on the craniums of all the men of obſervation in all ages; the celebrated phyſician Frank poſſeſſes it in an eminent degree, and Gall himſelf is very evidently furniſhed with it.

29. *Organ*

29. *Organ of the Talent for Satire.*

29. Satire and wit.

The organ of ſatire and facetiouſneſs (*witz* of the Germans, *wit* of the Engliſh, *facetiae* of the Latins) correſponds with the frontal protuberance. Gall preſerves many examples of the truth of this opinion, and we have uniformly found it true.

30. *Organ of Goodneſs.*

30. Goodneſs.

The organ of goodneſs is in the middle of the forehead, above that of compariſon (No. 26.) It forms that oblong elevation which is conſtantly found in the portraits of Chriſt and of Mary, painted by Raphael and Correggio, and contributes much to that expreſſion of mildneſs and goodneſs with which we are delighted; it is always ſeen in the craniums of men naturally good, and is wanting in thoſe of the miſchievous and vindictive *.

Among animals the roe-buck, the hind, the pigeon, &c. are provided with it; on the contrary, animals of prey, ſuch as the eagle, the ſtarling, the tiger, the fox, &c. are without it; in the latter caſe, the *os frontis*, inſtead of being round and elevated, is depreſſed and hollow.

31. *Organ of Muſic or of Theatrical Talent.*

31. Theatrical repreſentation.

The very marked enlargement of the ſummit of the *os frontis*, is owing to the developement of the organ for the repreſentation of thoughts by actions, the organ of muſic, or theatrical talent.

" Gall has collected many obſervations to prove the truth of this opinion, nor will it be overlooked inattentively, conſidering the heads of the great performers at the different theatres of Paris."

Note, " We think we have alſo obſerved that this organ is particularly developed in the deaf and dumb; and we attribute it to the neceſſity ſuch perſons are under of acting continually, an exerciſe which neceſſarily facilitates its advancement."

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* We are not here ſpeaking of that goodneſs which is the reſult of moral principles, that to which we allude exiſts as inſtinct, without being the produce of moral reflections.

32. *Organ of Theoſophy.*

The organ of theoſophy occupies the moſt elevated part of the *os frontis.* 32. Theoſophy.

All the portraits of ſaints which have been preſerved from former ages, afford very inſtructive examples, and if this character is wanting in any one of them, it will certainly be deſtitute of expreſſion.

It is *exceſſively* developed in religious fanatics, and in men who have become recluſe through ſuperſtition and religious notions.

It is the ſeat of this organ, which according to Gall, has determined men to conſider their gods as above them in a more elevated part of the heavens. In fact, when we conſider this ſubject philoſophically, there is no more reaſon for ſuppoſing that God is placed above the world, than there is to ſuppoſe him below it.

33. *Organ of Perſeverance.*

The laſt of the organs hitherto diſcovered by Gall, is that of perſeverance, of conſtancy, of character; it is ſituated in the anterior and ſuperior part of the parietals in the middle of the head; when it is in exceſs it cauſes obſtinacy, but its defect produces inconſtancy. 33. Perſeverance.

"With regard to thoſe parts of the ſkull in which Gall has not yet diſcovered organs, it is probable that his future inveſtigations will afford him the means of ſucceſs; on this ſubject, the work which he is about to publiſh, will give us more ample details. It will alſo be for him to convince us, by arguments perhaps inconteſtable, of the truth of his ſyſtem, the expoſition of which cannot be very ſatisfactory in a curſory outline."

We think it neceſſary alſo to remark, that the organs here enumerated are not diſtinctly perceptible, except in individuals who poſſeſs ſome faculty in an eminent degree, and that it is not poſſible to form a correct judgment of a moderate talent, on account of its organ being confounded among thoſe which ſurround it. "We ſee no reaſon, philoſophically ſpeaking, for the calumnies which have been lately thrown upon the ſyſtem of Gall, that *it tends directly to materialiſm.* When we admit of organs of the action of the internal faculties, the immeaſurable ſpace between mind and matter will continue the ſame. Objects of a nature ſo unlike, are incapable of union. On the other hand, the will of man continues unimpaired; it is this which muſt counterbalance the operation of the organs, morality ought to ſubdue the paſſions."

BOJAMES, M. D.

VII.

Method of conveying Boats or Barges from a higher to a lower Level, and the contrary, on Canals, by means of a Plunger, inſtead of loſing Water by Locks. By Lawson Huddleston, *Eſq. of Shaftſbury, Dorſet. Communicated by the Inventor.*

Conſtruction of a lock for raiſing and lowering boats.

WHEREVER there is occaſion to convey a commercial boat or barge from a higher canal to a lower (uſually termed a higher or lower line) or *vice verſâ*, a lock muſt be conſtructed of ſtone or other fit materials in the ſpace between the higher and lower canal, ſo as to communicate at the ends with both of them. The dimenſions of the lock are to be as follow; its horizontal ſuperficies or area ſhould correſpond both in form and ſize with that of the boat itſelf, with the allowance of ſufficient room only for the boat to riſe and ſink freely within it; and its depth and height ſhould be ſuch that the water within it may riſe with a loaded boat floating therein from the level of the lower to that of the higher canal: there muſt alſo be two ſluices one at each end of the lock, large enough to admit the free ingreſs and egreſs of the boat, one of which muſt be accommodated to the level of the higher canal, the other to that of the lower.

Reſervoir on one ſide of the lock,

On one ſide of the lock there muſt be a reſervoir of equal height with that of the lock, the form of which may be rectangular or not (though the former may perhaps be the more convenient) the area of which ſhould be equal to, or rather in practice ſomewhat exceeding, that of the lock. In *this* caſe the perpendicular depth of the reſervoir ſhould be equal to double the intended riſe or fall of the veſſel within the lock, but where the areas of the lock and reſervoir are *unequal* this proportion varies: for as in all caſes the quantity of water to be diſplaced out of the reſervoir into the lock muſt be equal in cubical content to that of the ſpace within the lock between the ſurface of the lower line and that of the upper, it follows that if the area of the reſervoir be greater than that of the lock the neceſſary depth of the water to be diſplaced will be ſo much the leſs than in the caſe before ſtated; and *e contra* if the area of the reſervoir be leſs than that of the lock the neceſſary depth of the water to be diſplaced muſt be ſo much the greater: ſo that whether this proportional quantity of water is to be obtained by the greater depth or the greater area of the reſervoir, is a point for the conſideration of the artiſt.

and communicating with it,

There muſt be a communication ſomewhere near the bottom between the lock and reſervoir, that the water in each may be always on a level; which level may by means of the lower ſluice always correſpond with that of the water in the lower canal, except during the actual operation of raiſing or ſinking a boat within the lock.

in which a plunger may be raiſed or depreſſed.

To the inſide of the reſervoir muſt be fitted a * ſolid body or plunger, in ſpecific gravity ſomewhat exceeding water, and of ſuch a bulk and form as will exactly fill the whole of the reſervoir, allowing only ſufficient room for the plunger to move freely up and down therein.

Method of operation: as the plunger ſubſides the boat riſes and the contrary.

Let us now ſuppoſe the reſervoir to be 16 feet deep, and by means of the communication with the lower canal to be preciſely *half* full of water. Let us alſo ſuppoſe the plunger to be ſo ſuſpended by machinery as to be movable up and down within the reſervoir, and to be barely above the ſurface of the water, when it, [the plunger] is at its greateſt height. And now let us ſuppoſe that a boat or barge is floated through the lower ſluice into the lock. That ſluice being now ſhut, as well as the upper one (which was all along ſuppoſed to be ſo) the plunger is let down to the bottom of the reſervoir: by this operation though the plunger be not in actual contact with the ſides of the reſervoir, much leſs what is called *water-tight*, (a neceſſary circumſtance in the caſe of forcing-pumps, piſtons, &c.) the water in the reſervoir will be forced into the lock, and thereby raiſe the boat or barge as much as the plunger ſinks, viz. 8 feet: in which ſituation the water in the lock being now on a level with that in the upper canal, the upper ſluice may be raiſed, and the boat floated out. The plunger being ſtill *down*, if a boat be floated into the lock from the upper canal, let the upper ſluice be ſhut and the plunger be raiſed, and the boat will in like manner deſcend in the lock to a level with the lower canal.

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* At leaſt ſo far ſolid as to outweigh its bulk in water.

Hydroſtatic principle.

It is obvious that the *principle* of this invention is founded on this law in hydroſtatics, viz. that two columns of water however different in lateral dimenſions, will, if there be a communication between them, always maintain one and the ſame level.

The plunger is counterpoiſed.

The plunger is to be counterpoiſed by a weight acting on a ſpiral curve (on the ſame ſhaft with the wheels or mechaniſm that raiſe the plunger) in order to accommodate the action of the counterpoiſe to the *decreaſing* weight of the plunger as it deſcends into the reſervoir; ſo that being *counterpoiſed* it may be eaſily raiſed or ſunk by means of the mechanical apparatus uſually employed for ſuch purpoſes, poſſibly indeed this movement may alſo be effected by means of a fire-engine, but the *expediency* of ſuch means is not meant to be ſuggeſted.

General remarks.

In theory the wall or partition common to both the lock and reſervoir may not be neceſſary, however uſeful it may be in practice.

Should occaſion require it, the reſervoir may be placed at any given diſtance from the lock, provided there be a communication between them, at or near the bottom.

Without ſuggeſting any preciſe limitation (which can be learned only from experience) the projector conceives that locks on this conſtruction are better adapted to low or moderate lifts than to high ones; boats alſo of a moderate ſize (as recommended by Dr. Anderſon and Mr. Fulton) and ſo conſtructed as to exceed rather in *depth* than in length or breadth, ſeem beſt adapted to the ſcheme now propoſed.

Advantages of this lock.

On a compariſon then between this lock and others of a late invention, it is preſumed that wherever it may be expedient to uſe it, it will be attended with the following material advantages.

No machinery except to the plunger;

1ſt. The only machinery neceſſary (the two ſluices excepted) in this lock, is for the purpoſe of raiſing and ſinking the plunger or plungers.

2d. Whilſt the machinery in other contrivances above alluded to, is either wholly or in part liable to injury from water, the machinery in this (the two ſluices as before excepted) is wholly exempt from this diſadvantage.

not liable to injury from the water;

3d. Whilſt caiſſoons, coffers, &c. require not only the opening of one or more ſluices, but alſo a previous fitting or adjuſting of their ends to the mouth of the canal to prevent the loſs of water, before the boat can enter the lock; according to this plan, on the opening of a ſingle ſluice the boat floats at once into the Lock without loſs of water, and without any ſuch delay or difficulty: when I ſay without loſs of water, it muſt be underſtood with this allowance, that if a boat goes down laden, and returns (or another goes up) unladen, the upper level gains a body of water equal to the weight of the cargo, *e contra* the lower canal gains ſimilarly if a boat goes down unladen and returns laden.

nor requiring cloſe fittings.

No loſs of water.

4th. If the water in the upper canal at any time be ever ſo abundant, no boat can paſs through the former locks but *by working the machinery;* whereas according to this plan the *machinery may be at reſt,* and the boats paſs on as through the common gate locks, whenever the upper canal can afford the neceſſary expence of water.

If water be abundant, the machinery need not be uſed,

Laſtly. If at any time the machinery of any *one* lock of the former kind (out of the many that may be neceſſary in a given length of canal) ſhould happen to be diſordered, the *whole navigation is at a ſtand,* whereas in a lock of this conſtruction in caſe of ſuch an accident, the boats may ſtill paſs and repaſs as through a common gate lock.

and therefore the navigation will not be impeded by any inevitable diſorder.

Let us ſuppoſe a boat (or barge) to be 20 feet long, and ſix feet wide, and to draw three feet water. The ſolid content will be 360 cubic feet, each of which weighing about 62 pounds, the burthen will be nearly 10 tons.

Particular ſtatement of dimenſions, &c.

Is it propoſed to raiſe (or ſink) this boat eight feet, by means of a plunger of equal area or horizontal ſuperficies with that of the boat.

The deſcent of the plunger muſt in *this* caſe be equal to the aſcent of the boat, viz. eight feet, and the height of the plunger muſt (as in *all* caſes) be equal to the ſum of both, viz. 16 feet; the ſolid content of the plunger therefore will be 1,920 cubic feet, amounting in weight to nearly 53 tons.

Let

Mechanic force ſaved by enlarging the horizontal ſection of the plunger.

Let us now ſuppoſe that *two* ſuch plungers are joined together laterally, ſo as to form a new plunger, of the ſame height, but of twice the area and weight of the former. It is plain that this new plunger (moving in a reſervoir adapted to its dimenſions) will by deſcending four feet, raiſe the boat in the lock as much as the other by deſcending eight feet, viz. eight feet:—and the momenta (conſiſting of weight multiplied by velocity) of both theſe plungers will be equal, becauſe the product of 53 [tons] multiplied by eight [the velocity] equals the product of 106 [tons] multiplied by four [the velocity] each amounting to 424.

But it will be found that, as in *all* caſes the height of the plunger is equal to the ſum of its own deſcent added to that of the boat's aſcent, the height of this new plunger, inſtead of amounting to 16 feet, will only amount to 12: its weight therefore will be only 79½ tons, and its momentum 318: it will therefore require ſo much the leſs time or labour to work it, in the ratio of 318 to 424, or three to four. Qu. Would not this objection ariſing from the neceſſity of having ſtronger machinery to ſupport this latter plunger, be counterbalanced by the advantage of working it with ſo great a ſaving of labour and time?

Remedy for depoſition of ſediment.

Should it be apprehended that, notwithſtanding the *forcing* effect of the plunger, the water in the reſervoir may gradually depoſit ſuch a quantity of ſediment at the bottom as to obſtruct the deſcent of the plunger, it is obvious, that there may be a depending drain under the reſervoir to be opened occaſionally, or the bottom of the reſervoir itſelf may be ſo much below the loweſt point of the plunger's deſcent, as to render the cleanſing it very ſeldom neceſſary *.

The machinery may be worked by waſte water.

It remains to be obſerved, that as every canal ought to be ſo well provided with water as to allow of *ſome* waſte (though not enough perhaps to ſupply the loſs of a lock-fall with every boat that paſſes) the machinery of the lock may be worked thereby, without the mechanical aid commonly employed for ſuch purpoſes; for if the plunger preponderates over the reſiſting medium, and counterpoiſe barely enough to overcome the friction of the axis, &c. it will at all times deſcend ſlowly within the reſervoir, and raiſe the boat, when left at liberty ſo to do; and if inſtead of being allowed to deſcend the whole eight feet required, it be ſtopt after having deſcended ſeven feet, the admiſſion of one foot depth of water from the upper line into the lock, may be ſufficient to raiſe the plunger again with the loſs of only $\frac{1}{8}$th part of a lock-fall of water.

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* The impetus with which the water could ruſh out of the reſervoir into the lock, by the action of the plunger, could probably raiſe and force out any ſediment, except ſtones or gravel.

Plate XII. Fig. 1. Exhibits a perſpective view of a model of the apparatus above deſcribed, having part of the front taken away in order to ſhew the interior parts. **View of the apparatus.**

A. Repreſents the upper ſluice of the lock.

B. The lower ſluice.

C. The plunger.

D. The ſnail upon which the weight or chain is wound that carries the counterpoiſe.

VIII.

An eaſy Method of Churning Butter: By CIT. JUMILHAC, *Preſident of the Society of Agriculture, of the Seine and the Oiſe*[*].

TO increaſe the powers of man, is one of the attributes of mechanics, but in order that a mechanical contrivance may be truly uſeful, it is requiſite that it it ſhould be ſimple and cheap. That which I now offer to the ſociety, appears to me to unite theſe two advantages.

It is univerſally known, that in great heats and extremely cold weather it is difficult to churn butter. The labour of ſeveral perſons ſucceſſively, is often applied without any ſucceſs, to procure the perfect coagulation of the cream. In vain during the rigour of winter, the cream is placed by the fire, or mixed with milk quite freſh and yet warm; and it is with as little ſucceſs, that in ſummer the churn ſtaff is occaſionally immerſed in cold water. All theſe means though of value, are inſufficient if not ſeconded by celerity of motion in the act of churning.

Surpriſed at the ſlowneſs with which butter is formed, eſpecially in winter, and after having continued the operation for five hours and a half, I thought I ſaw that the difficulty aroſe from the aukwardneſs and conſtraint of the arm that holds the churn ſtaff; both which are increaſed in proportion as the cream becomes thicker, as it is then neceſſary to make a greater exertion to raiſe and depreſs the churn ſtaff. This obſervation is ſo true, that a perſon who has churned butter for half an hour at the beginning, without experiencing any fatigue, cannot perform the ſame work for the ſpace of ten minutes after the cream has become thick and offers more reſiſtance.

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[*] Sonini's Bibliotheque Phyſ. Oeconomique, No. 1.

Convinced that every interruption, or even diminution of ſpeed, are highly injurious to the butyraceous coagulation, I thought that, without altering either the churn or churn ſtaff, I could adopt a method which is already uſed in ſeveral other operations. Some members of the ſociety think they have already ſeen this application to the churn ſtaff, but not finding it mentioned in any book, I have preſumed, that the ſociety will not object to make it ſufficiently public, in order that it may be adopted in all dairies.

Nothing more is required than to fix to one of the beams of the ceiling, a pole three yards long, ſupported at the diſtance of two feet by a croſs piece, and itſelf faſtened at the end by two blocks or cleats nailed to the timbers. This pole may be about three inches or more in diameter at the upper part, and half that thickneſs at the lower or moveable end. At the end of the pole is tied a ſtrong cord, which is paſſed through a hole made near the top of the churn ſtaff, which is ſo faſtened and placed, that it continues ſuſpended in the middle of the churn.

When the churn ſtaff thus fitted up is preſſed downward, it enters eaſily into the cream; but the pole acting as a lever by its elaſticity, raiſes it immediately; this eaſes the arms, for the moment of the greateſt exertion is when you are obliged to raiſe them as high as the forehead to diſengage the churn ſtaff, and immediately force it down again.

Even if this method contributed only to diminiſh the principal part of the aukwardneſs and conſtraint, it would be a great advantage: but I ſhould think this but trifling and incompleat if the contraction of the arm were not remedied. After having conſidered what powers are jointly employed to produce the effect, I concluded that the poſition of the arm ought not to be neglected during this long operation; and ſeeking by trials for the leaſt painful, I determined in favour of that in which the fore arm is employed in the whole motion.

I therefore perforated the handle of the churn ſtaff about twelve inches below the hole through which the cord paſſes that ſuſpends it to the pole, and I there faſtened two ſmall handles of wood of about two centimetres each, which are faſtened together by an iron pin; ſo that the handle of the churn ſtaff reſembles a croſs. The perſon who churns places his hands on the two handles, and by a ſimple preſſure, which is ſo light, that two fingers are ſufficient to force the churn ſtaff to the bottom of the churn, he may churn for two hours without experiencing any real fatigue. For the motion of the elevation which alone demands the greateſt exertion, is performed by the lever of the pole.

I made uſe of alder wood for the pole, as one of the moſt elaſtic and leaſt likely to warp.

We ſee from this deſcription,

1. How little it coſts to eſtabliſh the mechanical help, which only conſiſts in a pole of alder wood of three yards long, and two ſmall wooden handles adapted to the churn ſtaff.

2. How greatly it aſſiſts and increaſes the power of the perſon who churns, by obviating the moſt painful movement, by placing the hands naturally, ſo as to require the motion of the fore arm only, which is evidently the leaſt fatiguing.

I am convinced by the reſult of conſtant practice, that one third of the time is ſaved which is uſually employed in making butter.

IX.

Deſcription of a Machine for rooting up the Stumps of Trees. By CIT. SAINT VICTOR, *Member of the Society of Agriculture, for the Department of the Seine*[*].

Inconveniencies of leaving the ſtumps of trees after felling the trunk.

EVERY cultivator is well aware how detrimental the ſtumps of trees are which remain in the ground, which inſtead of being rooted up at the time they were cut down, have been ſawed off cloſe to the ſtump, to ſave the expence of digging them out of the earth.

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[*] Bibliotheque Phyſ. Oeconomique, de Sonini, No. 1.

Inſtance.

In an eſtate in Savoy, I was ſubjected to theſe inconveniences, from the avarice of the proprietor who had felled a great number of large trees, and ſawed them off cloſe to the roots, theſe were oaks, walnuts, and cheſnuts, which grew in the meadows and ploughed grounds, and he diſpoſed of them as timber in planks and fire wood, without taking any care to have them rooted up at the ſame time, which would have been then more eaſy, by the common practice of uſing the trunk as a lever.

Attempt to blow them up by gunpowder.

When I came into poſſeſſion of this eſtate, I foreſaw that I could by no means avoid rooting up a great number of ſtumps that were very injurious to every kind of cultivation; and to avoid the long, troubleſome, and expenſive labour of digging great trenches, which are uſually required to be made, in order to bring up the ſort of roots, I thought I might employ the force of gunpowder. This attempt perfectly ſucceeded, by means of a little machine of iron I had forged, of which I here join a plan, ſection, and perſpective view, which ſhews the manner of placing it beneath the ſtump *.

Deſcription of the Machine.

Machine for blowing up the roots of trees.

It conſiſts of a bar of forged iron, about two feet eight inches long, one inch thick towards the handle, and of two inches towards the breech or platform.

It is a ſmall mortar with a plug and handle.

The platform which is circular, is 14 inches in diameter.

This platform ſerves as the baſe of the chamber, or furnace of the mine, which is three inches in diameter, and three inches eight lines in the length of its bore.

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* The uſe of this ingenious machine ought not to be confined merely to the ſtumps of trees in our fields. Licentiouſneſs and avarice within the laſt five years, have deprived our country of a quantity of woods and thickets, and degraded by immenſe fellings, our nobleſt foreſts, the incalculable ſource of riches for the preſent, and hope for future time. Whether theſe ſpaces thus impoveriſhed be converted into arable ground or paſture, or whether the imperious neceſſity or claims of poſterity ſhould require that they ſhould again be planted with trees, the invention of Cit. Saint Victor, will be a moſt uſeful application to eradicate from the ground thoſe roots which would deprive us of a part of its product.

Note of the editor.

The ſtopper or tampion which ſerves as a plug to the mine, is of the ſame diameter, to enter within after a ſlight paper or wadding. It is attached by a chain to the gun or mortar, which laſt is eight inches in diameter.

About two inches above is added a ſmall touch hole and pan. The hole is directed in an angle of 45 degrees, and is primed with powder to communicate with the charge with which the chamber is filled up to the ſtopper.

This engine may be caſt even with more facility in braſs or bronze, and in this caſe, it muſt be a little thicker in all its dimenſions, in order to afford a reſiſtance equal to that of the forged iron.

Uſe of the Machine.

Application beneath the ſtump intended to be blown up.

When the machine is charged with powder, a ſmall excavation is made with a pick axe, in the center of the ſtump. The machine is then placed in it, ſo that the plug immediately touches the wood. Care muſt be taken to fill all the vacancies, either with ſtones, or pieces of iron, or wood, more eſpecially beneath the platform of the machine, in order that the exploſion of the powder may have its full effect on the ſtump, of which if neceſſary, the principal roots ſhould firſt be cut, if any appear on the ſurface of the ground near the ſtump, that is to be eradicated.

When the machine is firmly fixed in its place, the priming is put into the pan, a ſlow match applied, the length of which is ſufficient to allow time to retire to a proper diſtance from the exploſion.

By inſpection of the plan and ſection, every one will perceive the utility of this ſimple machine, and it may eaſily be made by any intelligent ſmith.

Delineation and plate.

A. Plate XIII. Fig. 1. Plan of the machine of about two feet three inches long.

B. Plan of the machine, 10 inches high, comprehending the plug.

a. The plug with its cap faſtened to the chain.

b. The chamber for the powder.

C. The touch hole.

The middle figure repreſents the machine placed under the ſtump of a tree *.

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* Mr. Knight, ironmonger, of Foſter Lane, has contrived a ſimple apparatus for ſplitting blocks of wood with gunpowder, of which the deſcription will be given in our next.—N.

Method

X.

Method of Secret Writing, by means of a Steganographic Scale. *By* J. B. BERARD *.

Methods of ſecret writing.

AN infinite variety of methods of ſteganography or indecipherable writing has been deviſed; of theſe, ſome are defective in theory, and others inconvenient in practice. It is not my intention to diſcuſs the advantages or inconveniences of all thoſe which are known, it would lead me too far. I ſhall therefore confine myſelf to noticing a few of the principal ones.

Alphabets of convention.

Alphabets of convention afford but little ſecurity; the characters which perform the functions of vowels and conſonants, recur in ſuch ſituations, as to make their uſe very apparent; and it is aſcertained, that by patience and a little ſkill the ſecret is eaſily diſcovered.

Sympathetic ink.

The ſympathetic inks, ſeven in number, are not more ſafe, becauſe after ſeveral trials, that re-agent which renders the writing legible, is at length diſcovered.

Folding the paper.

Thoſe methods which depend on folding the paper in a particular manner, as was practiſed by the Spartans, are inconvenient, and afford but little defence againſt curioſity.

Kircher's method.

The method of Kircher, though ſufficiently certain in its principle, is of little value in practice; beſides which, its performance is both tedious and inconvenient: a point wrong placed or omitted, is ſufficient to render the ſecret unintelligible to the correſpondent.

General enumeration.

In the ſixth number of the *Journal de l'Ecole Polytechnique*, p. 382, is a table, in which Cit. Haſſenfratz has claſſed all the modes of correſpondence, whether by writing or otherwiſe. That which I have deviſed, is as follows; it will perhaps be found to poſſeſs the double merit of ſimplicity and ſecurity †.

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* Melanges Phyſico-Mathematique.

† This method has been made uſe of for the private correſpondence of Cit. Forfait, miniſter of the marine, and the colonies, and preſident of the lyceum of arts. There is no inconvenience in mentioning the fact; for the advantage of a good method conſiſts in

Take a rule of paſteboard, wood, ivory, or copper, about one inch broad and ſeven inches long, divide its edge into 30 equal parts, and between the diviſions write the firſt thirty natural numbers, from 1 to 30 incluſive, in any arbitrary order. The following is the manner of uſing this rule.

New method by a ſcale of tranſpoſition.

Operation for writing. Firſt make a minute or outline of the ſecret you wiſh to ſend, then place the rule or ſteganographic ſcale on the paper intended for the ſecret, and mark the two ends of the rule by two ſmall lines of ink, the reaſon of which will be ſeen below.

The proceſs. To write.

This being done, tranſcribe the firſt thirty letters of the minute, writing the firſt letter oppoſite to the figure 1, on the rule, the ſecond oppoſite to the figure 2, the third oppoſite to the figure 3, and ſo on to the end.

Bring down the rule and tranſcribe in a ſimilar manner, oppoſite to the figures, the next thirty letters of the ſecret, that is to ſay, from the thirty-firſt to the ſixtieth incluſive.

Continue thus tranſcribing in new lines, each conſiſting of thirty letters, until the whole ſecret is written.

The punctuation muſt be carefully inſerted to the right of that letter which is on its left in the minute, and a mark like this + muſt be placed above the laſt letter of each word, to diſtinguiſh it from that which follows, and thus render the reading eaſy to the correſpondent.

Operation of the correſpondent. The correſpondent who receives the ſecret with the letters thus miſplaced, will be able to tranſcribe it, and replace the letters in their proper order, by inverſing the preceding operation.

To decypher.

For this purpoſe, he muſt be provided with a rule ſimilar to that made uſe of for deranging the letters of the ſecret. He will place it on the firſt line, ſo that the letters correſpond with the figures on the rule. This will be eaſily accompliſhed by means of the two lines of ink which mark the ends of the rule.

It will then be eaſy for him to tranſcribe the letters of the ſecret in their proper order, beginning with that oppoſite to

figure

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in its capability of being employed by all the world, without diminiſhing its ſecrecy for each individual; it will be ſeen that, in this plan, it is the ſecret of the ſeries agreed on, and not that of the method, which renders the correſpondence ſafe.

figure 1, next that oppoſite to figure 2, and ſo on to thirty he will then bring down the rule, and proceed in the ſame manner with the ſucceeding lines.

Example.

Example. Suppoſe this to be the ſecret to be ſent.

L'eſcadre mettra à la voile au premier vent favorable; elle ſe rendra à Toulon, où je lui enverrai des ordres ultérieurs.

Suppoſe the ſeries of the ſcale employed ſhould be this,

2. 4. 6. 8. 10. 12. 14. 16. 18. 20. 22. 24. 26. 28. 30. 1. 3. 5. 7. 9. 11. 13. 15. 17. 19. 21. 23. 25. 27. 29.

The anagrammatic writing or tranſpoſition of the ſecret, will be;

```
    +   +     + +                  + +
lecde e ta lv i e u r meL's a r mtrà a ol ape il
        +    + +     +      +               +
lvnfv r be; le e e d a Ture t a oale l sr nrà ol
      +     +               +  + +          +   +
loojl i ne ri e o d e u t l n, ù e u evr a ds rrs ll
lrer                      é i  u s.                d
```

Obſervations.

The combinations to be decyphered in one ſingle line, are many millions of millions,

1. If we wiſh to form a notion of the ſecurity of this method, let us conſider what a geometer muſt do who undertakes to explain the ſecret.

Every line contains a number of combinations, expreſſed by the product of the numbers 1. 2. 3. . . . . to 30. that is to ſay, a number expreſſed by 265 followed by 30 cyphers. This number of anagrams is equal to that of the grains of ſand which would encircle the whole world, ſuppoſing each grain to be leſs than one hundredth of an inch in diameter. This, without doubt, is more than ſufficient to diſcourage the moſt perſevering Newton, and it muſt be acknowledged that the difficulty may be conſidered as an abſolute impoſſibility.

and they are ſtill more taken all together.

But this is not all, after the labour of a multitude of ages, this geometer would be no farther advanced; for among the infinity of combinations produced from the thirty letters of one line of the ſecret, he would find an innumerable multitude that might yield a rational meaning, and by what means would he be able to diſcover which of them the author of the ſecret had in view? In ſhort, what muſt be the difficulty where it becomes neceſſary thus to combine all the lines of the ſecret at the ſame time? Here the imagination is loſt.

This method is adapted to *numerous correſpondents.*

2. In the event of many correſpondents, it would be eaſy for the greater ſecurity to agree upon a different ſeries with each, if the ſcale is made of paſteboard, it may be marked

with four ſeries, and may ſerve for four correſpondents: if it be of ivory, the ſeries may be changed at pleaſure, by writing the figures with a pencil: the ſcale may even be compoſed of thirty ſmall detached moveable pieces of braſs ſlipped upon a flat iron rod, and ſecured with a nut at one of its ends.

Other permutations.

3. It may be eaſily conceived, that the principles of permutation may be varied to infinity, for inſtance, by changing only the order of the ſyllables, the words, or the lines. The above method on which I have fixed, appears to be at once eaſy, certain, and conciſe, and to require the ſimpleſt apparatus.

4. If it be deſired to conceal even the appearance of a ſecret, the method above deſcribed may be employed to write with one of the ſympathetic inks, at the end of an oſtenſible ſubject.

Another method by the ſcale,

5. *Another method.* Having divided the paper into 100 or 200 ſquares, write in each a letter, a ſyllable, or a word of the ſecret, and alſo one of the numbers of the concerted ſeries: ſeparate and mix the ſquares; the correſpondent will only be required to replace them in their order by means of the figures of the ſeries.

For the greater ſecurity, the edges of the ſquares ſhould be pared to deſtroy the connection of the adjoining ſides.

another by intercalation.

6. *Method of intercalations.* Draw on paſteboard, ſome parallel diviſions about half an inch aſunder, pierce them with holes, either equal or unequal, of an arbitrary ſize, and at any diſtance from each other: lay the paſteboard on the paper, and write the ſecret by inſerting ſucceſſively from left to right, a letter, ſyllable, or word in each vacancy: remove the paſteboard and fill up the intervals with inſignificant letters; or it will be better, if time permit, that the letters ſhould form with thoſe of the ſecret, a reaſonable meaning, not liable to be ſuſpected.

The correſpondent will eaſily read the ſecret through holes in a ſimilar paſteboard.

The inſertion of unmeaning letters may be avoided by uſing three or four paſteboards of equal ſize, but ſo pierced, that when they are laid on each other, all the holes will be cloſed. By writing through each of theſe in ſucceſſion, all the lines will be filled. The correſpondent muſt be provided with ſimilar paſteboards, and will read the ſecret through them by applying them ſucceſſively to the writing.

This method is ſhorter than that of the anagrammatic ſcale, and is preferable when there is much to write; but it is leſs ſecure, and requires a more complicated apparatus.

XI.

Deſcription of a Magazine Piſtol, which when loaded is capable of being diſcharged Nine ſucceſſive Times through the ſame Barrel. W. N.

Account of a magazine piſtol.

I AM indebted to the liberality of the Right Honourable Lord Camelford for the accurate drawings of the curious and valuable piece which forms the ſubject of the preſent Memoir, as well as for permiſſion to uſe and examine it in any manner I might think proper. It is of German make; the workmanſhip very good, and it has been uſed by his Lordſhip without any particular care in various parts of the world. Its conſtruction does, in effect, ſhew that its uſe is attended with neither danger nor uncertainty; but I ſhall poſtpone my remarks till I have given the deſcription.

Deſcription by reference to the drawings.

Figs. 1 and 4 in the Plates XIV. and XV. exhibit views of the two ſides of the piſtol. Fig. 5. is a ſection to ſhew the magazines, and Figs. 2. and 3. ſhew what may be called the chamber piece. The large face of this laſt (Fig. 2.) is ſlightly taper or conical from X towards Y, where the diameter is ſmalleſt, and the ſmall part B is cylindrical, excepting that a portion is ſcooped out on one ſide marked by the dark ſpace D in Fig. 3. Its proper place in the piſtol is in the breech, croſſing the line of direction at right angles. When the lock is off, it goes into its cell from the ſide Fig. 1. which it fits with conſiderable accuracy, but does not come to rub hard, becauſe the Plate X forms a projecting face which ſtops it. In this ſituation its ſmall cylindrical part B projects outwards, and is received in an hole of the ſame diameter in that part of the lock beneath the hammer where the pan is uſually placed. In fact, the cavity D, Fig. 3. does conſtitute the pan when its poſition is ſuch as to be immediately beneath the covering face of the hammer. The ſcrew B which goes into the cylindrical piece, and Z, which goes into the ſtock, are the faſtenings by which the lock is ſecured. At the oppoſite end

A,

Deſcription of a piſtol which fires nine balls in ſucceſſion by once charging.

A, Fig. 2. the chamber piece terminates in a ſquare, upon which the broad head of the lever L, Fig. 4. is fitted, and kept down by the ſcrew A, which goes into the chamber piece. It is to be underſtood that the cock and hammer are conſtructed, and act in the ſame manner as in the beſt fire arms, and do not therefore require to be deſcribed. The lever is capable of being moved from the poſition M to thoſe of L and N; but is prevented from deſcribing the remaining part of the circle by an interior ſtop, which may be eaſily imagined without any attempt at minute explanation.

Fig. 5. being a ſection through the middle of the ſtock, breech, and part of the barrel, ſhews the relative ſituation of the magazines, with the chamber piece, and other parts. The balls S, 2, 3, 4, &c. are lodged in their proper receptacle, being put in through the hole R, Fig. 4. and the powder is lodged in its magazine Q Q. both which are cloſed by the door T, which is then faſtened by a ſmall bolt and back ſpring. A, Fig. 5. ſhews the ſituation of the chamber piece at the time when the lever is brought to the poſition M. This is done with the muzzle of the piece pointing towards the ground, and the effect is, that powder runs into the chamber P (ſee alſo Figs. 2 and 3) and a ball into the chamber S. The lever is then moved from M to L and N, by which proceſs the ball chamber drops its ball into the barrel as it paſſes oppoſite to N, where the ball remains, becauſe the actual bore of the ſcrew barrel is not wide enough to allow it to paſs farther than juſt to clear the moveable chamber. As ſoon as the lever has arrived at the poſition N, the powder chamber P is exactly oppoſite the ball, and ready to be diſcharged againſt it. After the diſcharge the muzzle is to be again depreſſed, and the lever moved from N to M: the chambers become again charged with powder and ball; and by returning the lever back to N, this ball and powder become duly placed for a ſecond diſcharge. It is obvious that theſe diſcharges may be repeated until all the balls have been fired out. The ſmall bridge in the powder chamber P (Fig. 2.) is to prevent any impediment from the entrance of part of one of the balls into the receptacle, and the perforations W, W, in the breech, ſerve to clear the ſurface of the chamber piece from any foulneſs it might acquire.

Thus

Deſcription of a piſtol which fires nine balls in ſucceſſion by once charging.

Thus far we have ſpoken only of loading and diſcharging; but this piece would admit of little rapidity of effect, if it did not at the ſame time cock and prime itſelf. It may be obſerved, that the projecting part, or ſtud C, is fixed to the chamber piece Fig. 2. and 3. a very little behind the ſhot chamber; ſo that it ſtands at the top of the lock at the time of charging. A thin flat bar of iron proceeds from the cock, and ſlides with it along the face of the lock plate, withinſide the hammer: againſt this plate the ſtud C acts, and brings it to full cock a little before the lever arrives at the poſition M; at the ſame time that the more prominent part of C preſſes on the back of the hammer, and ſhuts the pan. The lock is therefore put into the condition to give fire by the ſame ſimple operation, and preciſely at the ſame moment as the charge is taken from the magazines.

It now remains only to be ſhewn how the priming is given. The excavation D in the ſide of the ſmall cylinder B, Fig. 3. conſtitutes the pan, and the ſmall dot repreſents the touch hole paſſing through the metal into P the powder chamber. When the lever is in the poſition M, Fig. 4, the cavity D is exactly placed beneath the covering face of the hammer G: but at the time of charging it has the poſition Fig. 3. conſidered with regard to that of Fig. 1. In this laſt figure the dark ſhaded ſpace H denotes a reſervoir for priming, the door of which may be opened and ſhut under the action of an ingenious back ſpring, operating nearly like that of a claſp knife. A long perforation or ſlit communicates from this reſervoir to the ſpace in which the cylinder B revolves: ſo that when the excavation D paſſes that ſlit (during the return of the lever) it becomes filled with powder, which it carries round to the laſt poſition, which is exactly that in which it muſt receive the fire whenever the piſtol is to be diſcharged.

The hiſtory of this conſtruction ſeems to be imperfect; but there is reaſon to ſuppoſe that it is of ſome antiquity. The magazine air gun of *Colbe* conſtructed for carrying ten balls, and lodging them ſucceſſively in the barrel by a croſs cylindrical piece was made early in the laſt century, and is deſcribed in Deſagulier's Lectures, and moſt elementary books. In the preſent arm the contrivances are highly judicious, as well with regard to mechaniſm as to arrangement. If it were poſſible for the powder magazine to be ſet on fire at the diſtance of a ſemi-circumference of the chamber piece from the explosion, the only effect would be that the door would be blown open, and this is ſituated in a place where it could do no harm. The ſame remark is alſo applicable to the magazine for priming.

Deſcription of a piſtol which fires nine balls in ſucceſſion by once charging.

How great the advantages muſt be in battle, for a man to be able to reload his piece by a ſimple movement of one ſecond of time, without taking his eye off his enemy; or how conſiderably uſeful this invention might prove in the defence againſt robbers need not be ſtated. It can indeed be ſtated, that the opponent may alſo provide himſelf with the like advantage; and then we have only to urge the argument, that the duration of wars have been diminiſhed, and its humanity diminiſhed by rendering the means of annoyance more perfect.

I have diſcharged this courſe of balls ſeveral times, and I find that the whole nine balls can be fired in 30 ſeconds.

XII.

On the Diſſemination of Plants. *By* CIT. L. REYNIER *.

THE hiſtory of vegetables affords ſome facts to which I think it my duty to call the attention of naturaliſts: they relate to the diſſemination of plants, and to the means by which this is effected. I have already collected ſeveral obſervations on this ſubject in the dictionary of Agriculture of *L'Encyclopédie Méthodique*, article *Diſſemination.*

Diſſemination of plants,

There are two natural means of reproduction: one of theſe is by the roots, which, ſpreading outwards, form new ſtems round the mother plant; this reproduction is ſlow, and can only take place gradually and without intervals; the other is by the ſeeds, which being carried by the winds, or by the hooks with which they are provided, or by animals which ſwallow them, and afterwards depoſit them, unchanged, in their excrements, are removed to greater diſtances, though ſtill within a limited circle. It is not therefore wonderful, to ſee a plant ſpring up in a ſpot, where the ſame ſpecies is known to exiſt at no great diſtance; its introduction is in the claſs of poſſibilities.

by the ſpreading of the roots,

or tranſportation of ſeeds.

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* Decade Philoſophique, No. 13. An. XI.

But

New vegetations that appear when the face of the ground is diſturbed.

But that which ought to excite our attention, is the production of a new vegetation in a place which has undergone ſome very great changes. In all caſes when the ſurface of a place has been altered either by the falling or removal of the earth, by opening roads through foreſts, or by draining marſhes, plants are ſure to be found in the following year, which did not exiſt there before, while the former ſpecies diſappear, except a few coſmopolites, if the expreſſion may be allowed, which, though they re-appear, undergo a remarkable alteration *(Dict. d'Agr. de L'Encycl. meth.* art. *Climat.)* When a wood is grubbed up, the foreſt plants ceaſe to grow, and thoſe natural to tilled ground immediately germinate. Labat notices (ſee Vol. I. p. 386.) that, in his time, at the Antilles, as ſoon as the ground was cleared, its ſurface became covered with purſlane. In thoſe places where charcoal is made, in foreſts at a great diſtance from any habitation, I daily obſerve plants, different from thoſe which were formerly there, and which are natives either of dry paſtures or cultivated lands, ſuch as the (vergerette) of Canada, the annual veronicas, &c. Forſter, in his voyages with Cook, took notice of ſeveral iſlets in the middle of the ſea, ſimply rocks of coral, on which the firſt rudiments of vegetation were forming. In the low valleys of the downs of Holland, I have gathered a ſatyrion and an ophrys, natives of the ſummits of the Alps and the plains of Spitzbergen *(Satyrium viride, L. Ophrys monorchis, L.)* But theſe downs, according to the obſervations of Cit. Décandolle, *(Ann, d'Agr. T. XIII. p.* 372.) are not of very ancient origin. How is the introduction of theſe two plants, which are naturally provided with very limited means of multiplication, to be accounted for? I have expreſſed a wiſh in the Encyclopedie, that the new iſlands formed by volcanos, and particularly thoſe near Santorini ſhould be examined. One of our moſt accurate obſervers is on the point of viſiting the latter, *(See d'Olivier, T. II. p. 238).* The new one produced by the eruption of 1707 to 1711, does not yet ſhew any appearance of vegetation, the air is ſtill mephitic; that which aroſe in 1573 has ſome traces of vegetation, particularly ſome graſſes, and a ſmall fig tree; the latter may have been depoſited by ſome bird, but by what means were the graſſes conveyed?

Are theſe produced by the roots or ſeeds of other plants.

It is true that ſeeds which are buried to a certain depth will be preſerved for a great length of time, and being afterwards ſubmitted by culture to the action of the light and water, will germinate. But is it to be preſumed, that the ſeeds of purſlane were preſerved in the earth of the Antilles, during the many ages the foreſts had endured before they were deſtroyed? Is it probable that, in the foreſts of Gaul, whoſe antiquity is well aſcertained, the ſeeds of thoſe plants, which appeared after they were rooted up, or after placing a charcoal furnace in them, ſhould have been preſerved ſound from before the exiſtence of the foreſts? The diſſemination of plants has neceſſary limits; beyond theſe, we muſt have recourſe to other explanations.

Could the ſeeds have been ſo long preſerved.

Laſtly, the plants of the claſs cryptogamia, and particularly fungi, whoſe form is conſtantly determined by the nature of the ſubſtance on which they grow, and is conſtantly the ſame in ſimilar circumſtances, although they do not appear at ſtated ſeaſons like other vegetables, alſo preſent new facts. It is not long ſince Cit. Ventenat obſerved a boleta of a peculiar new ſhape, which was produced on a human body. I have noticed the clavaria, which is conſtantly formed on the cryſalides of caterpillars (*Journal de Phyſ. Année.* 1787); others grow on thoſe fruits which have a woody covering; ſuch as are formed on the fragments of fir, are not the ſame as thoſe which grow on the wood of the oak. The huſk of the nutmeg produces a fungus peculiar to itſelf, (*Ancienne Encycl.* art. *Muſcade*). Laſtly, the fungi which are formed on the wooden props of mines, cellars, &c. are not the ſame as thoſe produced by the ſame woods when they decay in the open air. It is difficult to conceive the diſſemination of ſeeds from one mine to another, particularly to thoſe which are opened in ſituations where no mines had exiſted before; nevertheleſs I have found all thoſe which I have examined in different countries to poſſeſs very nearly the ſame form.

Facts reſpecting the cryptogamia.

All theſe conſiderations may induce us to preſume, that nature is daily exerciſing the ſame powers which ſhe poſſeſſed at the creation, and it would be intereſting to examine the cauſes and aſcertain the means of execution; but this can only be the conſequence of long and diverſified inveſtigations, carried on by a great number of obſervers.

It is preſumed that plants continue to be formed without propagation.

An

XIII.

An Eſſay on the Declivities of Mountains. *By* RICHARD KIRWAN, *Eſq. L. L. D. F. R. S. and P. R. I. A.*

Introductory remarks.

AMONG the various cauſes to whoſe activity the planet we inhabit owes its preſent wonderfully diverſified appearance, ſome undoubtedly exerted their influence from its very origin, and others at ſubſequent periods; of theſe laſt one at leaſt, namely, the Noachian deluge, was univerſal in its operation, while the effects of many more were partial and local, ſuch as thoſe reſulting from earthquakes, volcanos, particular inundations, &c.

In a general ſurvey of the globe, it is only to general cauſes, whoſe operation was univerſal, that our attention can be directed; the effects of partial cauſes being the proper objects of the geological hiſtory of thoſe countries that were particularly affected by them.

But to diſtinguiſh cauſes of the former claſs from thoſe whoſe operation was more confined, it is neceſſary to diſcover ſome character by which their effects may unequivocally be diſcerned.

Now a general *uniformity*, or agreement in ſome particular circumſtance in every part of the globe, ſeems to be a ſure teſt of the operation of ſome general cauſe. The diſcovery of uniform appearances is therefore of primary importance in geological reſearches. In the preſent eſſay I ſhall confine myſelf to the inveſtigation of one inſtance of this ſort, namely, the *inequality of declivity* which the ſides or flanks of mountains exhibit in every part of the globe hitherto examined according to the points of the compaſs to which they face, and are expoſed.

All high mountains or hills have one ſide much ſteeper than the other.

That one part of almoſt every high mountain or hill is ſteeper than another, could not have eſcaped the notice of any perſon who had traverſed ſuch mountains; but that Nature in the formation of ſuch declivities had any regard to different aſpects or points of the compaſs, ſeems to have been firſt remarked by the celebrated Swediſh geologiſt Mr. Tilas, in the 22d vol. of the

Memoirs of Stockholm for 1760*. Neither Varenius, Lulolph nor Buffon in his Natural Hiſtory publiſhed in 1748, have noticed this remarkable circumſtance.

The ſteep ſide faces the low country. Tilas.

The obſervation of Tilas however relates only to the extreme ends, and not to the flanks of mountains; with reſpect to the former, he remarked that the *ſteepeſt* declivity always faces that part of the country where the land lies loweſt, and the *gentleſt* that part of the country where the land lies higheſt, and that in the ſouthern and eaſtern parts of Sweden they conſequently face the E. and S. E. and in the northern the W. The eſſential part of this obſervation extends therefore only to the general elevation or depreſſion of the country, and not to the bearings of theſe declivities.

The weſtern ſide is ſteepeſt. Bergmann.

The diſcovery that the different declivities of the flanks of mountains bear an invariable relation to their different aſpects, ſeems to have been firſt publiſhed by Mr. Bergman in his phyſical deſcription of the earth, of which the ſecond edition appeared in 1773. He there remarked that in mountains that extend from N. to S. the weſtern flank is the *ſteepeſt*, and the eaſtern the *gentleſt*. And that in mountains which run E. and W. the ſouthern declivity is the ſteepeſt and the northern the gentleſt, vol. 2d. § 187.

Inſtances. In Scandinavia.

This aſſertion he grounds on the obſervations related in his firſt vol. § 32, namely, that 1° in Scandinavia the Suevoberg mountains that run N. and S. ſeparating Sweden from Norway, the weſtern or Norwegian ſides are the ſteepeſt, and the eaſtern or Swediſh the moſt moderate, the verticality or ſteepneſs of the former being to that of the latter as 40 or 50 to 4 or 2 †.

The Alps.

2dly. That the Alps are ſteeper on their weſtern and ſouthern ſides than on the eaſtern and northern.

The Cordelieres.

3dly. That in America the Cordelieres are ſteeper on the weſtern ſide, which faces the Pacific Ocean, than on the eaſtern. But he does not notice a few exceptions to this rule in particular caſes which will hereafter be mentioned.

Remarks of Buffon,

Buffon, in the firſt vol. of his Epochs of Nature publiſhed in 1778, p. 185, is the next who notices the general prevalence of this phænomenon, as far as relates to the eaſtern and weſtern ſides of the mountains that extend from north to ſouth, but he is ſilent with reſpect to the north and ſouth ſides of the mountains that run from eaſt to weſt; nay, he does not ſeem to have had a juſt comprehenſion of this phenomenon, for he conſiders it conjointly with the general dip of the regions in which theſe mountains exiſt. Thus he tells us, vol. 1ſt, p. 185, that in all continents the general declivity, taking it from the ſummit of mountains, is always more rapid on the weſtern than on the eaſtern ſide; thus the ſummit of the chain of the Cordelieres is much nearer to the weſtern ſhores than to the eaſtern; the chain which divides the whole length of Africa, from the Cape of Good Hope to the mountains of the Moon, is nearer, he ſays, to the weſtern than to the eaſtern ſeas; of this however he muſt have been ignorant, as that tract of country is ſtill unknown.

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* See alſo vol. 25, Swed. Abhandl. p. 291, where *Cronſted* explains ſome obſcure parts of Tilas's obſervation.

† The verticality of the ſides is inverſely as the length of the deſcent.

The mountains which run from Cape Comorin through the peninſula of India are, he ſays, much nearer to the ſea on the eaſt than on the weſt; he probably meant the contrary, as the fact is evidently ſo, and ſo he ſtates it in the 2d vol. p. 295; the ſame he tells us may be obſerved in iſlands and peninſulas, and in mountains.

and other authors;

This remarkable circumſtance of mountains was notwithſtanding ſo little noticed, that in 1792 the author of an excellent account of the territory of Carlſbad in Bohemia, tells us he had made an obſervation, which he had never met with in any phyſical deſcription of the earth, namely, that the ſouthern declivity of all mountains was much ſteeper than the northern, which he proves by inſtancing the *Erzgebirge* of Saxony, the Pyrenees, the mountains of Switzerland, Savoy, Carinthia, Tyrole, Moravia, the Carpathian and Mount Hæmus in Turkey, 2, Bergm. Jour. 1792, p. 385, in the note.

Herman;

Herman in his Geology, publiſhed in 1787, p. 90, has at leaſt partially mentioned this circumſtance, for he ſays that the eaſtern declivities of all mountains are much gentler and more thickly covered with ſecondary ſtrata, and to a greater height, than the weſtern flanks, which he inſtances in the Swediſh and Norwegian mountains, the Alps, the Caucaſian, the Appenine and Ouralian mountains; but the declivities bearing a ſouthern or northern aſpect he does not mention.

La Metherie;

La Metherie, in the 4th vol. of his Theory of the Earth, of which the ſecond edition appeared in 1797, a work which abounds in excellent obſervations, p. 381[*], produces numerous inſtances of the inequality of the eaſtern and weſtern declivities, but ſcarce any of the northern and ſouthern, whoſe difference he does not ſeem to have noticed, but he makes a remark which I have not ſeen elſewhere, that the coaſts of different countries preſent ſimilar declivities.

With regard to eaſtern and weſtern aſpects, he thinks that a different law has obtained in Africa from that which has been obſerved in other countries, for in that vaſt peninſula he imagines the eaſtern declivities of mountains are the ſteepeſt, and the weſtern the gentleſt. Of this however he adduces no other proof, but that the greateſt rivers are found on the weſtern ſide: this proof ſeems inſufficient, as, if mountains be ſituated far inland, great rivers may flow indiſcriminately from any ſide of them, and ſometimes few rivers flow even from the ſide whoſe deſcent is moſt moderate, for inſtance, from the eaſtern ſide of the mountains of Syria; the Elbe and the Oder, two of the greateſt rivers in Germany, take their courſe from the weſtern ſides, the firſt of the Bohemian and the other of the Moravian mountains, which yet are the ſteepeſt. Many originate from lakes, as the Shannon with us; many take ſuch a winding courſe, that from a bare knowledge of the place of their diſemboguement it is impoſſible to judge from what ſide of a mountain they iſſue, if from any; their courſe at moſt diſcovers the depreſſion of the general level of the country.

J. R. Foſter: Univerſal fact: that the S. and S. E. ſides of mountains are ſteepeſt.

In 1798, the celebrated traveller and circumnavigator, John Reinhold Foſter, publiſhed a geological tract which merits ſo much more attention, as all the facts were either obſerved by himſelf, or related to him by the immediate obſervers. In this he ſtates as a fact univerſally obſerved, that the ſouth and ſouth-eaſt ſides of almoſt every mountain are ſteep, but that the north and north-weſt ſides are gently covered and connected with ſecondary ſtrata in which organic remains abound, which he illuſtrates by various inſtances, ſome of which have been already, and others will preſently be mentioned.

At preſent this fact attracts the greateſt attention, being obviouſly connected with the original ſtructure of the globe, and clearly proving that mountains are not mere fortuitous eruptions unconnected with tranſactions on the ſurface of the earth, as has of late been confidently advanced.

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[*] It is to be regretted that he ſcarce ever quotes his authorities.

I ſhall now ſtate the principal obſervations relative to this object, that have been made in different parts of the world.

In Europe.

Account of mountains: in Europe,

1. The mountains that ſeparate Sweden from Norway extend from north to ſouth, their weſtern ſides are *ſteep* and the eaſtern *gentle*, 1. Bergm. Erde Beſchreib. p. 157.

2. The Carpathian mountains run from E. to W. their ſouthern ſides towards Hungary are ſteep, their northern towards Poland moderate, Foſter, § 46.

3. Dr. Walker, profeſſor of natural hiſtory at Edinburgh, obſerved that the coaſts and hills of Scotland are ſteeper and higher on the weſtern ſide than on the eaſtern; Jamiſon's Mineralogy of Shetland, p. 3. However, Jamiſon obſerved, that the ſouth ſide of the iſle of Arran is the loweſt, and the north ſide the higheſt, p. 51.

4. The mountains of Wales are gentle on the eaſtern and ſteep on the weſtern ſides.

5. The mountains of Parthery, in the county of Mayo, are ſteep on the weſtern ſide.

6. The mountains which ſeparate Saxony from Bohemia deſcend gently on the Saxon or northern ſide, but are ſteep on the Bohemian or ſouthern ſide; Charpent, p. 75. The ſouthern declivity is to the northern as ſix to two, 2d Bergm. Journ. 1792, p. 384 and 385.

7. The mountains which ſeparate Sileſia from Bohemia run nearly from E. to W. yet are ſteeper on the northern or Sileſian ſide than on the oppoſite Bohemian; Aſſemanni *Sileſia*, 335. Such branches as run from N. E. to S. W. have their weſtern covered with primordial ſtrata, and conſequently leſs ſteep; 4. New Roz. p. 157.

8. The Meiſſener in Heſſia is ſteeper on the N. and E. ſides which face the Warra, than on the ſouth and weſtern; 1. Bergm. Journ. 1789, p. 272.

9. The mountains of the Hartz and Habichtſwald are ſteep on the ſouth and gentle on the northern ſides, Foſter, § 46.

10. The Pyrenees, which run from E. to W. are ſteeper on the ſouthern or Spaniſh ſide; Carbonieres, XIII.

11. The mountains of Crim Tartary are gentle on the northern and ſteep on the ſouthern ſides, Foſter, ibid.

In Aſia.

in Aſia,

12. The Ourals, which ſtretch from N. to S. are far ſteeper on the weſtern than on the ſouthern ſides; Herman Geologie, p. 90, and 2d Ural Beſchreib. p. 389.

13. The mountain of Armenic to the weſt of the Ourals is ſteep on its E. and N. ſides, but gentle on the ſouthern and weſtern; 1. Pallas Voy. p. 277.

14. The Altaiſchan mountains are ſteep on their ſouthern and weſtern ſides, but gentle on the northern and eaſtern; Foſter, ibid. and Herman 2d. Ural Beſchreib. p. 390, in the note.

15. So alſo are the mountains of Caucaſus, 3d. Schrift. Berl. Geſelſch. 471.

16. The mountains of Kamſkatſka are ſteep on the eaſtern ſides, Pallas, 1 Act. Petropol. 1777, p. 43.

17. The Ghauts in the Indian peninſula are ſteep on the weſtern ſides.

18. The mountains of Syria which run from N. to S. ſkirting the Mediterranean, are ſaid to be ſteeper on the weſtern ſide facing the Mediterranean; 4. La Metherie, p. 380.

In America.

in America:

The Cordelieres run from N. to S. their weſtern flanks towards the Pacific are ſteep, their eaſtern deſcend gradually.

In Guiana there is a chain of mountains that run from E. to W. their ſouthern flanks are ſteep, their northern gentle; Voyages de Condamine, p. 140.

Deduction of cauſes.

To aſſign the cauſes of this almoſt univerſal allotment of unequal declivities to oppoſite points, and why the greateſt are directed to the weſt and ſouth, and the gentleſt on the contrary to the eaſt and north, it is neceſſary to conſider,

1. That all mountains were formed while covered with water.

The earth wa originally covered with water.

2. That the earth was univerſally covered with water at two different æras, that of the creation, and that of the Noachian deluge.

Two æras.

3. That in the firſt æra we muſt diſtinguiſh two different periods, that which preceded the appearance of dry land, and that which ſucceeded the creation of fiſh, but before the ſea had been reduced nearly to its preſent level; during the

former, the primæval mountains were formed, and during the laſt, moſt of the ſecondary mountains and ſtrata were formed.

4. That all mountains extend either from E. to W. or from N. to S. or in ſome intermediate direction between theſe cardinal points, which need not be particularly mentioned here, as the ſame ſpecies of reaſoning muſt be applied to them, as to thoſe to whoſe aſpect they approach moſt.

In the firſt the primæval mountains were formed, and the waters moved from E. to W. and from N. to S.

Theſe preliminary circumſtances being noticed, we are next to obſerve that during the firſt æra, this vaſt maſs of water moved in two general directions, at right angles with each other, the one from E. to W. which needs not to be proved, being the courſe of tides which ſtill continue, but were in that ocean neceſſarily ſtronger and higher than at preſent: the other from N. to S. the water tending to thoſe vaſt abyſſes then formed in the vicinity of the ſouth pole, as ſhewn in my former eſſays. Before either motion could be propagated, a conſiderable time muſt have elapſed.

The primæval mountains reſiſted the waters, which occaſioned the greateſt depoſitions on the N. and E. ſides.

Now the primæval mountains formed at the commencement of the firſt æra, and before this double direction of the waters took place, muſt have oppoſed a conſiderable obſtacle to the motion of that fluid in the ſenſe that croſſed that of the direction of theſe mountains. Thus the mountains that ſtretch from N. to S. muſt have oppoſed the motion of the waters from E. to W. this oppoſition diminiſhing the motion of that fluid diſpoſed it to ſuffer the earthy particles with which in thoſe early periods it muſt have been impregnated, to cryſtallize or be depoſited on theſe eaſtern flanks, and particularly on thoſe of the higheſt mountains, for over the lower it could eaſily paſs; theſe depoſitions being inceſſantly repeated at heights gradually diminiſhing as the level of the waters gradually lowered, muſt have rendered the eaſtern declivities or deſcent, gentle, gradual, and moderate, while the weſtern ſides receiving no ſuch acceſſions from depoſitions, muſt have remained ſteep and craggy.

Again, the primæval mountains that run from E. to W. by oppoſing a ſimilar reſiſtance to the courſe of the waters from N. to S. muſt have occaſioned ſimilar depoſitions on the northern ſides of theſe mountains againſt which theſe waters impinged, and thus ſmoothed them.

Where

Mountains near each other would intercept this; depoſition.

Where mountains interſect each other in an oblique direction, the N. E. ſide of one range being contiguous to the S. W. flanks of another range, there the afflux of adventitious particles on the north-eaſt ſide of the one, muſt have frequently extended to the S. W. ſide of the other, particularly if that afflux were ſtrong and copious; thus the *Erzgebirge* of Saxony, which run from W. to E. have their N. E. ſides contiguous to the S. W. ſide of the *Rieſengebirge* that ſeparate Sileſia from Bohemia, and hence theſe latter are covered with the ſame beds of gneiſs, &c. as the northern ſides of the Saxon, and thereby are rendered ſmooth and gentle comparatively to the oppoſite ſide, which being ſheltered, remains ſteep and abrupt, which explains the ſeventh obſervation.

It would be leaſt at the greateſt heights, &c.

The cauſes here aſſigned explain why the covering of adventitious ſtrata on the higheſt mountains is generally thinneſt at the greateſt height, and thickeſt towards the foot of the mountain, for the bulk of the water that contained the adventitious particles being proportioned to its depth, and the maſs of earthy particles with which it was charged, being proportioned to the bulk of water that contained them, it is plain, that as the height of water gradually decreaſed, the depoſitions from it on the higher parts of the mountains muſt have been leſs copious than on the lower, where they muſt have been oftener repeated.

Hence, 2. granitic mountains, generally the moſt ancient, frequently have their northern or eaſtern ſides covered with ſtrata of gneiſs or micaceous ſhiſtus, and this often with argillite, or primæval ſand-ſtone, or lime-ſtone, theſe being either of ſomewhat later formation or longer ſuſpendible in water.

Hence, 3. different ſpecies of ſtone are often found at different heights of the ſame flank of a mountain, according as the water which conveyed theſe ſpecies, happened to be differently impregnated at different heights; during the firſt æra its depoſitions formed the primitive ſtony maſſes, but after the creation of fiſh, lime-ſtone, ſand-ſtone, ſarcilites and ſecondary argillites, in which piſcine remains are found, were depoſited. But during the ſecond æra, viz. that of the Noachian deluge, by reaſon of the violence and irregularity of its aggreſſion, the depoſitions were more miſcellaneous and are found at the greateſt heights; yet in general they may well be diſtinguiſhed by the remains of land animals, or of vegetables, or of both, which they preſent in their ſtrata (or at leaſt by the impreſſions of vegetables which they bear) as theſe muſt have been conveyed after the earth had been inhabited. But mountains regularly ſtratified bearing ſuch remains, for inſtance the carboniferous, cannot be deemed to have been formed in a period ſo tumultuous. During this deluge the waters alſo held a different courſe, proceeding at firſt from ſouth to north, and afterwards in both oppoſite directions, as ſhewn in treating of that cataſtrophe in my ſecond eſſay.

Exceptions from local cauſes.

Hence, and from various contingent local cauſes, as partial inundations, earthquakes, volcanos, the eroſion of rivers, the elapſion of ſtrata, diſintigration, the diſruption of the lofty mounds by which many lakes were anciently hemmed in, ſeveral changes were produced in particular countries that may at firſt ſight appear, though in reality they are not, exceptions to the operations of the general cauſes already ſtated.

Inſtances.

Thus the mountains of Kamſkatſka had their eaſtern flanks torn and rendered abrupt by the irruption of the general deluge, probably accompanied by earthquakes. And thus the Meiſſener had its E. and N. flanks undermined by the river Warra, as Werner has ſhewn; thus the eighth and ſixteenth obſervations are accounted for, as is the thirteenth, by the vaſt inundations ſo frequent in this country, 1 Pallas, p. 172, which undermined or corroded its E. ſide, while the weſtern were ſmoothed by the calcareous depoſitions from the numerous rivers in its vicinity.

Different ſpecies of ſtone muſt be found on different ſides of mountains.

Hence, 4. we ſee why on different ſides of lofty mountains different ſpecies of ſtones are found, as Pallas and Sauſſure have obſerved, 2 Sauſſ. § 981, a circumſtance which Sauſſure imagined almoſt inexplicable, but which Dolomieu has ſince happily explained, by ſhewing that the current which conveyed the calcareous ſubſtances to the northern, eaſtern, and north-eaſtern ſides of the Alps, for inſtance, was ſtopped by the height of theſe mountains, and thus prevented from conveying them to the ſouthern ſides, and thus the north-eaſtern ſides were rendered more gentle than the oppoſite, 3. New Rotz. p. 425. conformably to the theory here given.

Interceptions.

Hence, 5. where ſeveral lofty ridges run parallel to each other, it muſt frequently happen that the external ſhould intercept the depoſitions that do not ſurmount them, and thus leave the internal ridges ſteep on both ſides.

Low mountains.

Hence, 6. low granitic or other primitive hills are frequently uncovered by adventitious ſtrata on all ſides, as at Phanet in the county of Donegal, or are covered on all ſides; the impregnated waters either eaſily paſſing over them, or ſtagnating upon them, according to the greater or leſs rapidity of its courſe, and the obſtacles it met with.

Remarks on the ſtatements of Buffon and Bergmann.

The twofold motion of the ancient ocean is noticed both by Buffon and Bergman, but neither of them have deduced from it the true explanation of the phenomena of which we here treat: Buffon attributes the formation of ſecondary mountains to depoſition or ſediments from the ſea after the exiſtence of fiſh, 1. Epoques, p. 143, in 8vo. which he ſays inveſted the baſes of mountains without noticing any diſtinction of ſides, p. 144 and 170. He thinks theſe ſediments were equally conveyed from both poles towards the equator, for it is the equatorial regions that he thinks thoſe mighty caverns opened, towards which the primitive ocean was impetuouſly borne and in which it was ingulphed, p. 181, 182, and 183. If ſo, ſimilar declivities ſhould be formed on the ſouthern as on the northern ſides of mountains, which is contrary to the obſerved facts. His explanation of the eaſtern and weſtern declivities is defective and erroneous, for he attributes the abruptneſs of the weſtern ſides to the eroſion of the coaſts on that ſide (an eroſion that exiſts only in fancy) and the ſmoothneſs of the eaſtern to the gradual deſertion and retreat of the ſea on that ſide, p. 184. and 185, a retreat equally fictitious, as De Luc has well ſhewn. Whereas ſince the general motion of the ſea is from E. to W. if the eroſion were of either ſide it ſhould rather be on the eaſtern than on the weſtern; beſides, if the gentle declivities of the eaſtern ſides of mountains aroſe from the gradual retreat of the ſea, the petrifactions of the ſecondary mountains thus formed ſhould conſiſt of ſuch ſhell-fiſh as inhabit ſhallow ſeas or ſhores, whereas they conſiſt chiefly of thoſe called *pelagicæ*, which inhabit the greateſt depths *.

With reſpect to the eaſtern and weſtern declivities, Mr. Bergman's account of the origin of their inequality agrees exactly with mine, 2. Bergm. Erdeklotet § 183 and 187, but he fails in accounting for the inequality of the northern and ſouthern, for he ſuppoſes the courſe of the water to tend equally from both poles towards the Æquator which would render the depoſitions equal on both ſides, which is contrary to obſervation.

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* 2. Bergm. Erdekugel, p. 315.

XIV.

A Memoir on Animal Cotton, or the Inſect Fly-Carrier. By M. BAUDRY DES LOZIERES, *Member of ſeveral Academies, and Founder of the Society of Sciences and Arts, at Cape Francois.* (American Tranſ. V.)

GENTLEMEN,

Preface.

BEFORE I enter upon the ſubject of this memoir, I ought to pay the tribute of praiſe which is due to due to your uſeful labours. But the ſtyle of eulogy is ill ſuited to the plainneſs of an American farmer, and while you are conſtantly employed in *deſerving* praiſe, you cannot ſpare time to *hear* it.

Propoſed new branch of Weſt India commerce.

I am now going to communicate to you, with ſome obſervations upon it, a fact of entomology which I have myſelf witneſſed during my reſidence at St. Domingo, and which, if I am not miſtaken, deſerves your greateſt attention, becauſe it may introduce a new branch of commerce with the Weſt India colonies, and render very uſeful an animal which has hitherto been known only by the miſchief which it occaſions.

Every Inhabitant of the Weſt Indies knows and dreads the greedy worm which devours their indigo and caſſada plantations. But people have hitherto turned their attention more to the means of deſtroying it than of rendering it uſeful. It is indeed very natural to endeavour to deſtroy our enemy, but to compel him to be of ſervice to us is by far the greater triumph.

Its Birth, Growth, and Death.

Production, growth, and death of the caſſada worm.

The caſſada worm is produced like the ſilk worm, that is to ſay, from the eggs which the mother ſcatters every where, after ſhe has undergone her metamorphoſis into a whitiſh butterfly, or of a light pearl colour.

The egg is hatched about the latter end of July. Its developement is quick, for in September the worm is changed into a butterfly.

This month of September is the ſeaſon of his loves. The conſtant motion of his wings ſhews the ardency of his paſſion which he indulges day and night and even while feeding. The exceſs of this indulgence ſoon deſtroys him, he dies in the ſame month after violent convulſions.

I have ſaid that his life begins at the end of July. He is decked at his birth with a robe of the moſt brilliant variegated colours. This elegant livery, which nature ſeems to have delighted in forming, renders him always agreeable to the eye, which always dwells upon it with pleaſure.

Its Affinities.

External Appearances, &c.

It has appeared to me to be a ſmooth caterpillar whoſe external ſhape is exactly like that of the ſilk worm.

It differs however from it, by its ſize, by its thickneſs, and by the beauty of its colours.

It again differs from the ſilk worm, becauſe it does not itſelf work the cone which I am going to ſpeak of.

I leave it to the learned to delineate its external configuration, and to determine upon the family of inſects to which it belongs. I ſhall only ſay that I do not believe it has, like the ſilk-worm, an inteſtine going in a direct line from the mouth to the anus, becauſe it appears to me that this cauſe of elaboration would not have the ſame deſtination.

Its Food.

Food.

It feeds on caſſada leaves, of which it is extremely greedy. It feeds at all hours, day and night. It alſo nibbles the leaves of the potatoe, this is however but a tranſitory taſte, it ſoon returns to the caſſada leaf.

I have to obſerve that after it has taken its food, when the time of its metamorphoſis arrives, it does not purge itſelf by diet, like the ſilk worm, but continues to eat to the laſt moment.

The Approach of its Metamorphoſes.

Time and manner of its change

In the month of Auguſt, and when on the point of undergoing its metamorphoſis, it ſtrips off its ſuperb robe, and puts on one of an admirable ſea-green, this fundamental colour reflects all its various ſhades, according to the different undulations of the animal, and the different accidents of light.

The Sting of the Ichneumon Fly.

Immediate depoſition of thouſands of eggs in its body.

This new decoration is the ſignal of its tortures. Immediately a ſwarm of ichneumon flies aſſail it. I think I am not miſtaken when I aſſert that there is not one of its pores that has not one of thoſe flies faſtened to it. There is even no neceſſity of making uſe of the microſcope to ſee that he is covered with them.

In vain he ſtruggles with all his might, raiſes himſelf upright to get rid of his cruel tormentors—He muſt ſubmit. Thoſe flies, of the ſmalleſt ſpecies, and which can only be ſtudied by means of the microſcope, drive their ſtings into the ſkin of their victim, over the whole extent of his back and ſides. Afterwards, and all at the ſame time, they ſlip their eggs into the bottom of the wounds which they have made.

After having performed this dreadful operation, the ichneumon flies diſappear, and the patient remains for an hour, in a drowzy and even motionleſs ſtate, out of which he awakens to feed with his former voracity. Then he appears much larger, and his ſize increaſes every day. His green colour aſſumes a deeper hue, and the tints produced by the reflection of the light are more ſtrongly marked. The animal in this ſtate of factitious pregnancy, if I may ſo expreſs myſelf, is worthy of all the attention of the obſerver of nature.

I ſhall not undertake the deſcription of the ichneumon fly, it is well deſcribed in the books. If I have obſerved a difference, it is the ſame which exiſts between the European *gnat* and the *muſquitoe* of hot regions, that is to ſay, that our Weſt-India flies are of a leſſer ſize.

I have now to deſcribe the operation which the ichneumon flies, which are extremely ſmall, perform at the very moment of their coming into the world; you will judge, gentlemen, whether this expreſſion is accurate.

Animal Cotton.

Fibrous productions called animal cotton.

A fortnight after the ichneumon flies have thus cruelly depoſited their eggs by perforating the unfortunate caſſada-worm, that is to ſay, ſome time in the month of Auguſt, thoſe eggs may be ſeen by the help of a microſcope, hatching on the body of that animal.

History of the inſects.

Thoſe eggs are all hatched at the ſame moment, and it is impoſſible to catch the moral point of time which may intervene between the birth of one and that of another. At one glance, the caſſada-worm is ſeen covered with all the little worms that have juſt been hatched. They iſſue out of him at every pore, and that *animated robe* covers him ſo entirely, that nothing can be perceived but the top of his head. He then turns to a dirty white, the little worms appear black to the eye, but their true colour is a deep brown.

This operation laſts hardly more than an hour, and is followed by another which is not much larger but which is much more curious.

As ſoon as the worms are hatched, and without quitting the ſpot where the egg is which they have broke through, they yield a liquid gum, which by coming into contact with the air, becomes ſolid and ſlimy.

At the ſame time, and by a ſimultaneous motion, they raiſe themſelves on their lower extremity, ſhake their heads and one half of their bodies, and ſwing themſelves in every direction. Now is going to begin an operation which will afford the greateſt delight to the admirer of nature.

Each of thoſe *animalculæ* works himſelf a ſmall and almoſt imperceptible cocoon in the ſhape of an egg, in which he wraps himſelf up. Thus, they make, as it were, their winding ſheet. They ſeem to be born but to die.

Thoſe millions and millions of cocoons, all cloſe to each other, and the formation of which has not taken two hours, form a white robe in which the caſſada-worm appears elegantly clothed. While they are thus decking him, he remains in a ſtate of almoſt lethargic torpidity.

As ſoon as this covering is woven, and the little workmen who have made it have retired and hid themſelves in their cells, the worm endeavours to rid himſelf of thoſe barbarous gueſts, and of the robe which contains them, but he does not ſucceed in this attempt without the greateſt efforts.

He comes out of this kind of encloſure, entirely flaccid and dull, inſtead of his former fat and ſhining appearance, his ſkin now appears flabby, wrinkled and dirty, and gives him the appearance of decrepitude. He is now an exhauſted, ſuffering being, threatened with approaching death.

He will ſtill gnaw a few leaves, but he no longer eats with that voracious appetite, which indicated an active and vigorous conſtitution. Shortly afterwards he paſſes to the ſtate of a chryſalis, and after giving life to thouſands of eggs, he ſuddenly loſes his own, leaving to the cultivator who has not yet bethought himſelf of calculating the advantage that he may draw from him, an advantage which may be ſo improved as to much more than compenſate the ravages which he occaſions.

Shell of the Ichneumon Fly.

Shell of the ichneumon fly.

I had imagined that the thouſands of little worms which this ſhell contains in the cocoons of which it is compoſed, would be hatched ſome day. I ſhut it up therefore in a box cloſed with great caution. Every morning, and very often in the courſe of the day, I examined it, in order to catch the moment when thoſe little animals were to be born a ſecond time.

In fact, at the expiration of about eight days, I found the inſide of the box lined with a cloud of little flies. I made myſelf certain that they iſſued out of the little cocoon. Several which iſſued out of them before my eyes, left me no doubt as to the fact.

I then took up ſome of thoſe flies, and putting them on a pincer, I examined them with a microſcope.

They are bold and lively: they have four wings. Their antennæ are long and vibrating, their belly hangs by a very fine thread: there are ſome that have a tail, and others that do not ſhew it. Afterwards I ſatisfied myſelf that they feed upon ſmall inſects that appear to be of the family of *Acarus.* Thoſe indications appeared to me ſufficient to be ſatisfied that they belong to the family of the ichneumon.

Obſervations on Animal Cotton.

The animal cotton is white and pure;

I have often held in my hand that cotton ſhell or wrapper. Its whiteneſs is dazzling. As ſoon as the flies have quitted the cocoon, it may be uſed without any preparatory precaution. It is made up of the pureſt and fineſt cotton.

I call it *cotton* becauſe it is *idio-electric* and is pervious to the electric fluid.

conducts electricity;

I add to this denomination the epithet *animal,* in contradiſtinction to common cotton, which may henceforth be called *vegetable cotton,* ſo that the two ſpecies may be diſtinguiſhed from each other by their names, as they are by their origin, although they are very nearly related to each other in their effects.

It is to be obſerved, that what might be called *cob-web* in the covering of the fly-carrier, or ſmall flocks of ſilk which are probably intended to ſhelter the animal from the rain, is far ſuperior to what is called *ferrit* before, and *ſleet ſilk* after the preparation of the finer ſilk. There is no refuſe, no inferior quality in animal-cotton. Every thing in it is as fine and beautiful as can be imagined.

the whole is uſeful.

It is poſſible, if we may form a judgment by analogy, that medicine, which has extracted from ſilk what is called *Engliſh drops,* a remedy to which the greateſt efficacy is attributed, may derive a ſimilar advantage, perhaps for the cure of other diſorders, from an extract of the animal cotton, which might be called the *St. Domingo drops.*

In ſhort there is no need here of any of the precautions which the ſilk-worm requires. The robe which covers the fly-carrier, is worked every where, and every where perfectly well.

I ſhall only obſerve that as the rain ſpeedily deſtroys the caſſada-worm, the inſtant might be ſeized on when the ichneumon fly has depoſited her eggs, to put the fly-carrier under ſhelter. His natural food might be procured for him, as is done with the ſilk-worm.

Caution againſt rain.

The ichneumon fly never fails thus to come and depoſite her eggs. I have never ſeen a fly-carrier that was not covered with the robe or ſhell that I have ſpoken of. I have continued this obſervation for many years, and the crop was ſo abundant, that I alone, could collect in leſs than two hours, the quantity of one hundred pints, French meaſure.

I repeat it, animal cotton is attended with none of the difficulties which occur in the preparation of vegetable cotton. It is ſo pure, that as ſoon as the ichneumons have left it, which happens eight or ten days after their recluſion, it may be carded and ſpun.

Animal cotton judged to be much ſuperior to the vegetable.

If it ſhould want any preparation, it could be only in caſe it ſhould not have been ſufficiently guarded againſt duſt and rain.

Vegetable cotton, beſides the ſeeds that produce it and with which it is charged, is filled with extraneous matter, of which it cannot be freed, but with a minute attention, many hands and much time, or with the help of machines which have not yet been brought to perfection.

In every point of view, animal cotton appears to me to have a great ſuperiority over that of the vegetable kind.

It will, perhaps, be wondered at, that experience has not long ago aſcertained this fact, but let it be conſidered that the ſilk-worm and its uſe, were known long before any uſe was made of them, and that we are now carefully repairing the loſſes that we have ſuffered by the careleſs indifference of our fore-fathers.

The fly-carrier may experience the ſame fate, becauſe it is leſs difficult to reaſon than to make experiments, but I dare hope that as ſoon as it ſhall have prevailed over the ſophiſtry of indolence, it will ſtand the competition with ſilk and vegetable cotton. It is more abundant than either. It requires leſs time and leſs trouble to procure it.

This cotton makes good lint.

I have but one word more to add. Silk and vegetable cotton ſerve only to envenom and inflame wounds, which is attributed to the aſperities of their filaments; I have frequently employed animal cotton as lint in the hoſpital of my plantation, it has always ſupplied the want of that made of flaxen linen, and I have not obſerved the ſmalleſt inconvenience to ariſe from the uſe that I have made of it.

Had it not been for the troubles that have laid our colony waſte, and which have prevented the neceſſary communication, I ſhould have brought to you a fly-carrier in every one of the periods of his life. You would have ſeen the eggs, the magnificent robe with which he is decked at his birth, the kind of food that he is fond of, the ſimple but noble veſtment in which he wraps himſelf up on the approach of his tormentors, you would have ſeen thoſe covering his whole body as it were with points, you would have ſeen him covered with his ſhell, and that ſame ſhell carded, ſpun and ready for the weaver. I had in a great degree already executed this deſign.

M.

But it is too well known that I have not been able to ſave any thing in my flight from home, you will, however, be able at a future day to aſcertain the truth of the fact that I have ſtated to you. I thought that a fact of this nature deſerved to be depoſited among your archives, and I may perhaps requeſt of you the permiſſion of depoſiting there ſome other ſtill more curious facts.

Bт. DES LOZIERES.

Philadelphia, Feb. 3, 1797.

XV.

An Eſſay on the Fecula of Green Plants. By Profeſſor PROUST.

HILAIRE ROUELLE is the firſt who diſcovered a ſubſtance in the green fecula analogous to the gluten of flour. This ſubſtance, ſince that time, has never appeared doubtful, becauſe there are few chemiſts who have not ſeen its true characters. The fecula, of which it is the baſis, in the opinion of Fourcroy, is only an imaginary being, or, at moſt, it has been too ſlightly examined to be admitted in the number of immediate vegetable products; he even proceeds ſo far as to ſuppoſe that the albumen, that animal product, which no one ever before ſuſpected to be contained in plants, is the ſubſtance which ought to be admitted inſtead of the glutinous part of green feculæ.

Opinion of Fourcroy that the fecula of plants contain albumen and not gluten, as Rouelle aſſerted.

Are albumen and gluten found together or ſeparate in the juice of plants? This is the queſtion I have propoſed to ſolve, and I ſhall endeavour to reſolve it in this part of my obſervations on his "Syſtem of Chemical Knowledge."

are both found in plants?

To ſave my readers the trouble of turning to the work, I ſhall copy the paſſage in which the author has collected the facts and arguments upon which he has formed his judgment. This paſſage is ſtill more remarkable for the difference it preſents between his manner of characteriſing other vegetable products, and that of the chemiſts of the preſent day.

"Rouelle the younger, who examined and particularly compared it with animal matters, aſſerts that he has found it in the coloured feculæ, and eſpecially in that which is called the green fecula of plants. But the name of fecula being

Quotation from Fourcroy.

given indifferently to the fibrous matter contained in the juice of plants, and to the ſtarch, has induced chemiſts to conſider the latter as part of the reſidue of ſolid vegetable ſubſtances, and there is reaſon to think that it was only by analogy, or perhaps by certain equivocal properties, that Rouelle believed the green matter contained any of the glutinous ſubſtances; ſubſequent experiments at leaſt, and ſuch as I have many times repeated on coloured feculæ, have not yielded me the confirmation of this aſſertion, nor has any thing been aſcertained with certainty, that the gluten is one of the principles of this latter fecula."

The name of fecula, ſays Fourcroy, being given *indifferently* to the fibrous matter contained in the juice of plants, and to the ſtarch, has induced chemiſts to conſider the latter as part of the reſidue of ſolid vegetable ſubſtances, and there is reaſon, &c.

He accuſes the ancient chemiſts of inaccuracy,

I ſhall firſt notice that this opinion is not correct. For example, the chemiſts of the preſent day will never agree with Fourcroy, that the confuſion ariſing from the improper application of terms, of which later chemiſts have juſtly complained, has, by a neceſſary conſequence, produced inaccuracy in thoſe who have preceded us. Our maſters, it is true, gave bad names to things, but they did not confound them more than we do.

which is not well founded.

Even at the time when every vegetable precipitate was called a fecula, the ſimilarity of terms never miſled them ſo far as to cauſe them to confound the ſtarch with the reſidue of the ſolid parts of plants. Firſt, we are not acquainted with any reſidue of this deſcription, to which chemiſts can reaſonably compare it; and ſecondly, if any of them did take the green fecula for a reſidue, there was not one of them who did not perfectly know the difference between this fecula or reſidue and ſtarch; and as no ſuch confuſion is to be found in their works, the reproach is unjuſt; we need only look into thoſe of Rouelle, Macquer, Baumé, Sage, Parmentier, &c. to be convinced that the term of fecula has never miſled theſe authors into aſſimilations ſo diſcreditable to their judgment, as to place in the ſame rank the green fecula, the reſidue of the ſolid parts, and the ſtarch.

The fecula was never confounded with fibrous matter.

We will now proceed to the green feculæ, and affirm, that in the laboratories in pharmacy, and ſtill leſs in the hands of a chemiſt ſo celebrated for accuracy of obſervation as Rouelle, the broken fibres of green plants have never been confounded with that beautiful ſoft liquid expreſſed from their leaves, or with that emulſive product which paſſes pure through the ſtrainer, and which, by its exceſſive fineneſs and the brilliancy of its colour, differs ſo very much from the herbaceous filaments.

Its ſtate of diffuſion in water is not from grinding.

Again, if it were true that the fecula were a body homogeneous with the reſt of the plant; if it were poſſible to conſider it as in no reſpect different from the other parts, but by being more bruiſed, would it not be poſſible, by compleating the trituration of the remainder, to convert it alſo into fecula? When a freſh plant is bruiſed by the peſtle, it is broken ſmall; its texture is deſtroyed, but it is not pulverized.

This cruſhing for a few moments, differs too much from a dry pulverization, to admit of any compariſon between the fecula ſo produced, and a moiſtened powder. When an aqueous juicy plant, a ſedum for example, is cruſhed with a roller on the ſlab, its expreſſed juice will afford fecula. Certainly it is not to the trituration, that a fecula is indebted for its ſoftneſs, its fineneſs, and the impalpability which diſtinguiſhes it from powders. It is molecular in its own nature, and is even, perhaps, cryſtallized in thoſe fibrous cavities where it is depoſited by vegetation.

The doctrines of Rouelle were not mere aſſertions, but clear inveſtigations from numerous facts.

Rouelle aſſerts, according to Fourcroy, that the feculæ contains a principle ſimilar to animal matters, &c. Rouelle does more than that: little contented with ſimple aſſertions, he proves it, not by *analogy* or *equivocal properties*, but by a ſucceſſion of convincing facts, by approximations which have been univerſally admitted, becauſe they combine together the moſt prominent characters which were then known, or are even yet known, to exiſt in animal ſubſtances. Whence otherwiſe could Rouelle have drawn his analogies, to enable him to compare, as he does, the green fecula with the gluten of wheat? In fact, what is there in the common appearance of theſe two products that can lead to the compariſon? Their points of compariſon muſt be ſought for in their compoſition, in their chemical properties, and this was done by that indefatigable chemiſt. Theſe are approximations drawn from analyſis which ſerve as the baſis of the Memoir he has written on the green feculæ, and of which there is no mention in the Syſtem of Chemical Knowledge; doubtleſs becauſe in the

which Fourcroy has not detailed.

opinion of its illuſtrious author, Rouelle had confounded albumen with the gluten, and the detail of his ſuppoſed miſtake was conſidered as a matter of indifference in the hiſtory of chemiſtry.

Rouelle firſt diſcovered the peculiar animalized ſubſtance of the green fecula,

Rouelle however found in the fecula of ſorrel, a product ſo amply poſſeſſing the chemical properties of albumen, that he particularly inſiſted upon it in order to fix the attention of his time upon a ſubſtance ſo animalized; and as he afterwards obtained it from a plant, which, according to Fourcroy, does not yield the ſlighteſt trace of albumen, it is now inconteſtable, as it was then, that Rouelle was the firſt who diſcovered in the juices and green feculæ a *product* which, if not intitled to the name of albumen, poſſeſſes, nevertheleſs ſo ſtrongly, all the properties by which the attention of chemiſts has been called excluſively to it, that it is no leſs proper to be urged in the hiſtory of their diſcoveries, than albumen itſelf.

and the identity of veg. gluten and cheeſe.

It is to the ſame penetrating eye, the ſame impulſe of genius which led him to anticipate diſcovery, that he is indebted for that of the aſtoniſhing reſemblance between caſeum and gluten, when they have both undergone that ſpecies of fermentation which transforms them into the cellular combination, the odorous and ſavory compound, called cheeſe. And the gluten, in this ſingular reſult, reſembles the other more completely the more carefully it has been waſhed. Macquer, when he publiſhed what continues to be every where repeated, that part of theſe changes were occaſioned by ſome remains of ſtarch, was incorrect in his notion. Starch, a ſubſtance always inactive in fermentation, in that of bread, of beer, and even in germination, could only retard the effect produced by the gluten itſelf, and conſequently could only deſtroy, in part, thoſe characters, from which Rouelle ſtated the reſemblance of theſe two products.

Peculiar fermentation of cheeſe.

And even their analyſis extends far beyond the limits aſſigned to them; for when the gluten has changed its inſipid and viſcous mucoſity for the cheeſy ſtate; when it has gone through all the ſtages of that fermentation which is eſſential to that condition, it is found alſo to have acquired the taſte of thoſe ſharp and burning ſalts which conſtitute the principal merit of the Roquefort cheeſe; ſalts which have nothing in common with what is added, but are found equally powerful in the curd which has been waſhed and left to its own fermentation.

Salts generated in cheeſe, &c.

In fact in the cheeſe from gluten, as well as in that from animals, potaſh and ſulphuric acid enable us to find that ammonia, and that vinegar diſcovered by Vauquelin. Is the ammoniacal acetite then one of the ingredients which gives the flavour to cheeſe? I only know that alcohol applied to ſtrong cheeſe deprives it of all its taſte. An analyſis of that ſpecies might afford curious reſults; but let us return to the green feculæ; examine them by the lights of modern chemiſtry, and endeavour more particularly to diſcover whether albumen actually exiſts where Beccari and Rouelle found gluten.

Green Fecula.

The green fecula very concreſcible by heat.

I. The fecula undergoes, by heat, a change capable alone of giving it a deciſive character as to its nature. I mean that concreſcibility which has ſo few examples in vegetable products; that agglutination which compreſſes the particles together, and produces the appearance of a cheeſy curd. Though the fecula, before this change, paſſed eaſily through the ſtrainer, it can no longer do ſo after having been heated; a peculiar hardening has deprived it of its tenuity; but heat does not coagulate the fibrous tiſſue; in this reſpect the fecula does not reſemble the broken ſtraw of green plants.

Elaſtic and horny when dry.

II. The fecula, ſeparated from the juice by filtration, acquires an elaſtic and horny conſiſtence by drying. It is ſoftened with difficulty in heated water, but will not become ſoft even at the end of a month; notwithſtanding it is moiſtened it ſtill retains its horny ſtate. When bended it will return to its ſhape, and cannot by any means be crumbled: theſe qualities alſo are not found in the dry woody pulp.

More concreſcible than white of egg;

The feculæ of green and white cabbages, creſſes, hemlock, &c. do not loſe the property of coagulating by heat from that cauſe. Into water heated to between 50 and 60 degrees, plunge two matraſſes of equal ſize, one containing diluted fecula, and the other with the water and white of an egg; the fecula will harden and be collected in flocks, ſuch as are formed in a juice which is clarified by heat; but at this temperature the albumen will not even loſe its tranſparency.

III. The green fecula is very nearly in equipoiſe with water, for that of plants which are not acid, frequently requires a week to precipitate.

precipitable from water by alcohol and by acids.

Put ſome fecula waſhed and diluted into three glaſſes of equal ſize. To the firſt add a little alcohol, a few drops of acid to the ſecond, and place the third between them for compariſon. In the two firſt the precipitation will be compleat in half an hour, while in the third it will be hardly begun; alcohol and the acids therefore coagulate feculæ; but they have no ſuch action upon the woody reſidue.

Alcohol takes up near one ſixth of reſinous matter.

IV. One hundred parts of the dried fecula of hemlock yielded to alcohol from 15 to 16 of green reſin. After repeated infuſions to which it was ſubjected, it remained of an earthy grey, and the alcohol was incapable of bleaching it. Sage, who was well acquainted with feculæ, found that they yielded about one third of their weight in reſin; in order to ſeparate it eaſily, the fecula muſt be thrown into ſpirits of wine while yet moiſt, the fluid then penetrates and attacks all its parts; but the effect is much more difficult when it has become horny by drying.

(To be continued.)

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Prize Queſtions of Foreign Learned Societies.

National Inſtitute. Prize queſtions.

THE National Inſtitute of France held a public ſitting on the 20th Vendemiaire, when the new ſubjects for prizes were announced.

The *Claſs of Mathematical and Phyſical Knowledge* propoſed the following queſtion:

Winter ſleep of animals.

"To determine, by anatomical and chemical obſervations and experiments, what are the phenomena of inactivity which certain animals, ſuch as marmots, dormice, &c. undergo in the winter, with regard to the circulation of the blood, the reſpiration and the irritability; to inquire what are the cauſes of their ſleeping, and why it is peculiar to theſe animals."

This queſtion was propoſed before, and the prize was to have been decreed in this ſitting; but the meeting being of opinion that the memoirs received did not contain ſufficient information, decreed that it ſhould be propoſed again, and that the prize ſhould be doubled. It will be of the value of two kilogrammes of gold (about 6.800 fr.); and will be diſtributed at the public ſitting of Vendemiaire in the year 13. Memoirs muſt be tranſmitted to the Secretary of the Inſtitute before the 15th Meſſidor of the year 12.

The queſtion propoſed by the *Claſs of Moral and Political* knowledge is,

Decompoſition of thought.

" To determine how the faculty of thought can be decompoſed; and what are the elementary faculties which can be diſcovered in it?"

The prize is a gold metal of the weight of five hectogrammes (about 1700 fr.): it will be adjudged at the public ſitting of Germinal in the 12th year of the Republic.

The works cannot be received after the 15th Nivoſe of the ſame year.

Geography prize.

Prize for Geography. " To compare the geographical charts by Ptolemy, of the interior of Africa, with thoſe which have been tranſmitted to us by ſubſequent geographers and hiſtorians, with the exception of Egypt and the coaſts of Barbary from Tunis to Morocco."

This ſubject had been propoſed in the year 9, but the memoirs ſent not correſponding to the conditions of the notice, it was renewed.

The prize is a gold medal weighing five hectogrammes (about 1700 fr.), and will be adjudged in the public ſitting of Meſſidor in the year 12. The memoirs muſt be ſent in before the 15th Germinal of the ſame year.

Med. Soc. of Montpellier. Prize queſtion

The Medical Society at Montpellier has propoſed two prizes of 500 francs each, of which the firſt is to be adjudged in May 1803, and the ſecond in May 1804, on the following queſtions:

Inflammation

1ſt. In what kinds of diſeaſes, and under what circumſtances, is inflammation favourable or dangerous? and, in the treatment of ſuch diſeaſes, in what caſes ought it to be excited or checked.

Medicines by friction?

2d. To aſcertain by experiment and obſervation, what degree of confidence can be placed on the uſe of certain ſubſtances, by friction, which are generally adminiſtered internally; to determine the effect of ſuch remedies, in both methods, and alſo the quantity of the doſe; to point out the diſeaſes and caſes in which one method is to be preferred to the other; and finally, to determine, in the different maladies, to what parts of the body the application of theſe remedies, are moſt efficacious.

Flaſh

Flaſh from an Air-Gun.

Luminous flaſh from an air gun.

M. Pictet, in a letter from Paris of the 1ſt of January laſt to Mr. Tilloch (Philoſ. Magazine, Vol. XIV. 363.) ſtates a communication to the National Inſtitute of France on the 29th of December by M. Mollet, of Lyons, reſpecting the luminous appearance produced by the diſcharge of an air-gun in the dark, a phenomenon which he conſiders as never having been before obſerved. It has, however, been known for ſome time in this country, having been firſt mentioned, I believe, near a year and a half ago by Mr. Fletcher at a meeting for philoſophical experiments and converſations, which was then held weekly at my houſe. Several diſcuſſions accordingly took place with regard to its cauſe,—as, whether it was produced by electricity, or the change of capacity in the expanding fluid, &c. and it was intended to inſtitute a courſe of experiments on the ſubject, but ſome other objects of enquiry intervening the matter was poſtponed. It is a curious phenomenon, and deſerves inveſtigation.

Letter from Profeſſor PROUST *to J. C.* DELAMETHERIE.

On the Sugar of Grapes, and the Compound Nature of Urée.

New ſugar in the grape.

I have diſcovered a new ſugar in the grape, which is the baſis of wine; it is different from that of the ſugar-cane, cryſtalliſes differently, &c. It is contained in the proportion of about 30 per cent. in the juice of the grape. Azote is uniformly combined with the carbonic acid in the fermentation of wine: in that of gluten it is pure hidrogen, which is diſengaged with the carbonic acid.

The urée is a compound containing ammonia.

Tell Vauquelin that the urée, in the ſtate in which they have examined it, is a ſaline ſubſtance ſaturated with ammonia, and not a ſimple product; he need only apply ſulphuric acid to carry off this ammonia, and to have the urée pure, but coloured with a reſin from which I have not yet been able to free it.

J. de Phyſique.

On the Uſe of Sulphate of Soda in the Manufactory of Glaſs.
By PAJET-DESCHARMES [*].

Since the notice which was given in the *Journal de Phyſique* that ſulphate of ſoda might be uſed, without preparation or any intermediate agent, in the fabrication of ſiliceous glaſs, Cit. Pajot-Deſcharmes has thought fit to publiſh the reſult of his principal experiments on the employment of this ſalt in glaſs-making.

Uſe of ſulphate of ſoda in glaſs making.

It appears from the experiments which this operator has reported in the *Journal de Phyſique*,

1. That ſulphate of ſoda and ſand alone, in various proportions cannot ſucceed.

2. That ſulphate of ſoda, mixed with pounded charcoal, in the proportion of a tenth or twentieth part, yielded a yellow glaſs more or leſs black, of the nature of obſidian paſtes or ſtones: the crucibles were then very ſlightly acted upon.

3. That equal parts of carbonate of lime, dried ſulphate of ſoda, and ſand, produced a beautiful glaſs, clear, ſolid, and of a pale yellow: the crucibles were then very little corroded.

Cit. Pajot-Deſcharmes obſerves, that notwithſtanding the pains he took, he could never obtain a glaſs with ſulphate of ſoda that was not of a yellowiſh green, whereas muriate of ſoda (ſea-ſalt), treated in the ſame manner as ſulphate of ſoda, always produced a glaſs of a light blue tinge inclining more or leſs to green.

Cit. Pajot-Deſcharmes propoſes to treat more fully, in a particular memoir, on all the experiments he has made on this ſubject.

Obſervations on the Neceſſity of immerſing Seeds in Water in Times of Drought. By Ant. ALEXIS CADET-DE-VAUX.[†]

Franconville-la-Garenne, 2 *Brumaire* (*Oct.* 23.)

To reap, we muſt ſow; and, from the drought which has prevailed for ſix months, ſowing is not eaſy, for our gardens

In dry ſeaſons the ſeed grain is in danger of periſhing in the ground.

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[*] Abridged with remarks in the Journal des Mines, No. 69, from a Letter from Cit. Pajot-Deſcharmes, to J. C. de la Metherie, in the Journ. de Phyſique, Vol. LII. p. 210.

[†] Decade Philoſophique, No. 4. An. XI.

are no longer cultivated with the ſpade and the harrow, but with the mattock and the pick-axe; the plough, however, on account of its ſtrength, can ſtill be employed to till the ground, except in compact lands, and thoſe which are ſtiffly bound. But it is not enough that the land ſhall have been tilled to enable us to ſow it, the ſeed muſt alſo germinate, without which it dries and periſhes, or becomes the food of animals and inſects. For without rain, or dew which moiſtens at leaſt the ſurface of the ſoil, there can be no germination. In the mean time the ſeaſon advances, and the ſeed-time is already late. Let us point out, then, to the huſbandman, a method of preventing the inconvenience of drought; it is, not to commit his ſeed to the earth, until it is impregnated with the moiſture neceſſary for its germination.

Remedy; to ſoak it firſt in water.

We may refer to a great example: the Chineſe do not depoſit a ſingle ſeed in the earth until it has been immerſed in water. And I will adduce an experiment nearer home, which is in favour of this practice. Five years ago, I ſowed half an acre (*arpent*) of land with wild ſuccory, lucern, and pimpernel. With a view to compare the produce of theſe plants, I ſowed them in rows two feet aſunder. The ſpring was very dry, and I ſoaked each of the ſeeds in water for forty eight hours. The quantity ſoaked was not ſufficient; for there was required as much more as to ſow nine rows, three of each kind. I took advantage of this circumſtance to compare the effect ariſing from the ſeed being prepared or not prepared by immerſion. The reſult was, that in the nine rows ſowed with the dry ſeed, not more than thirty plants came up in five months: while the remainder of the land was covered, and formed a moſt beautiful artificial meadow. The rain fell too late to ſow theſe nine rows, and it was neceſſary to ſow them again in autumn.

Experimental proof.

Let us now apply theſe facts to the ſowing of corn.

Liming recommended; by immerſion.

The huſbandman is in the habit of liming his wheat when he apprehends the rot. This year he has no need to dread that evil; nevertheleſs let him uſe the lime, but by immerſion; for in general the method of application is defective. The operation is uſually confined to a ſimple ſprinkling of the heap of wheat with lime water, while it is turned over with a ſhovel.

The good, the only way of liming, is by immerſion: put the ſeed into tubs, and cover it to the height of four or five fingers breadths with lime-water, made ſo hot that the hand cannot be kept in it without difficulty; cover it up, ſtirring it three or four times in the twenty-four hours; after which draw out the bung that the water may run off, the quantity will be but ſmall; it will be nearly all abſorbed by the grain, which muſt be taken from the tubs, ſpread out in the air and then ſowed.

Method deſcribed.

Twelve buſhels of wheat, immerſed for twenty-four hours, will abſorb nearly one-fourth of water, that is to ſay, they will ſwell to fifteen or ſixteen buſhels by meaſure. Let us now inveſtigate the theory. Every grain of this corn carries with it to the earth a quantity of water, more than ſufficient to enſure its germination. This water acts principally upon the extractive matter of the huſk; it diſſolves this principle, one of the properties of which is to attract and ſtrongly to retain moiſture. Hence this water will not be evaporated. If inſtead of pure water, we uſe dunghill water, which is ſaturated with this extractive matter, together with deliqueſcent ſalts, and fatty matter, then the moſt minute quantities of ſurrounding humidity will be attracted towards the grain. But in truth it will, after this treatment, ſucceed very well without the ſpeedy aſſiſtance of the rains and dews; it poſſeſſes a ſufficiency of moiſture to put forth its germ, to throw out its radical, and in ſhort, to ſecure its germination. The grain which has been ſteeped gains, in ordinary ſeaſons, from twelve to ſixteen days in advance before that which has not been ſteeped; and in times of exceſſive drought, it gains every thing. If ſteeped, it germinates and grows; and if not ſteeped it dries and periſhes. Let the rains come, let them continue, ſtill I adviſe immerſion; which by forwarding the germination, remedies the inconveniencies of a late ſeed-time.

Wheat becomes one fourth larger by abſorbing water.

Dunghill water is beſt.

Recapitulation.

Extract from a Memoir by CIT. FOURCROY, *on the Chemical Nature of Ants, and on the ſimultaneous Exiſtence of two Vegetable Acids in theſe Inſects.*

Samuel Fiſcher firſt deſcribed this acid in 1670. It has ſince been more particularly examined by Margraff, Arvidſon, Bergman, &c. and finally by Cit. Deyeux, who eſtabliſhed the identity, which Margraff had previouſly ſuſpected

Experiments ſhewing the exiſtence of malic as well as formic acid in ants.

Experiments ſhewing the exiſtence of malic as well as formic acid in ants.

to exiſt between the formic and acetous acids. Neverthelefs there were ſtill ſome doubts to remove, and it was theſe which induced the Citizens Fourcroy and Vauquelin to undertake the following inveſtigations:

Some red ants (*formica rufa*, Lin.) were cruſhed in a marble mortar. A ſharp vapour was diſengaged, ſimilar to that of radical vinegar; and the alcohol, in which the ants were put to macerate, was tinged yellow.

This infuſion produced, by diſtillation, an inflammable liquor, ſlightly acid. At the ſame time it formed a browniſh ſediment, which was carefully ſeparated. This ſediment became covered with an acid liquor, which was ſaturated with lime.

The latter combination became brown and thick: it had a pungent nauſeous taſte, and the air produced bubbles in it, as in ſoap-ſuds.

One part of this compound, mixed with one part and a half of ſulphuric acid and two of water, produced a very thick magma, which, by diſtillation, yielded an acid liquor, without colour, of an empyreumatic odour, but which did not diſcover any trace of ſulphuric acid.

This acid combined with potaſh, formed a true acetite.

The browniſh thick compound, of which we have ſpoken above, formed, by ſolution in acetite of lead, an abundant precipitate, which proves that the acid extracted from the ants by the alcohol, contained ſomething beſide the acetous acid.

The ſame calcareous compound mixed with a ſolution of nitrate of lead, yielded an abundant yellow precipitate, which, treated with ſulphuric acid weakened by water, formed a new precipitate, heavier and whiter. The ſupernatant fluid was ſlightly acid and ſugary: it precipitated abundantly the nitrates of mercury, of ſilver, and of lead.

Many other facts, in addition to thoſe we have mentioned, are ſufficient proofs that the malic acid is joined with acetic acid in the liquor which is extracted from the ants by the alcohol; and it is doubtleſs the preſence of this acid which has led thoſe chemiſts into an error, who formerly treated of this ſubject.

The ants, after having been ſeparated from the alcohol, yielded, by diſtillation, an empyreumatic ſetid oil, carbonate of ammonia and acetite of ammonia, the whole diſſolved in a great quantity of water.

The brown ſubſtance left from the diſtillation of the infuſion in alcohol, was inſoluble in water, and ſoluble in alcohol, except a ſmall quantity of a browniſh matter, which appeared to the authors to be albumen. The ſolution of this brown ſubſtance in alcohol became milky by the addition of water; and, in a few days, depoſited a precipitate of a reſinous appearance, which ſeemed to be a fat matter of a peculiar nature.

Laſtly, the reſiduum from the ants was an animal carbon, which, after combuſtion, left only phoſphate of lime.

The memoir finiſhes with obſervations on the preſence of the acetous and malic acids in ants in particular, and in organic bodies in general.

Bulletin des Sciences, No. 70.

An Eſſay on the Relation between the ſpecific Gravities and the Strengths and Values of Spirituous Liquors; with Rules for the Adaptation of Mr. Gilpin's Tables to the preſent Standard, and two New Tables for finding the Per-centage and Concentration when the ſpecific Gravity and Temperature are given. By ATKINS and Co. *Mathematical Inſtrument Makers. Quarto, 74 Pages. London, Cadell and Davies, and Robinſons,* 1803.

Atkins on the ſpecific gravities of ſpirituous liquors.

It is certainly very remarkable, that no mode of denomination by which the real ſtrength of ſpirituous liquors could be defined with any tolerable degree of correctneſs, has ever yet been in general uſe in this or any other country. The miſchiefs accruing from the want of ſuch a ſyſtem, and the diſputes which were daily ariſing between the revenue officers and the traders, with reſpect to theſe matters, induced the legiſlature ſeveral years ſince to paſs an act by which was declared, that all ſpirits ſhould be deemed and taken to be of the degree of ſtrength which ſhould be denoted by Clarke's hydrometer. The inconveniences, however, which reſulted both from the complex conſtruction of the inſtrument itſelf, and the ineligibility of the mode by which the ſtrengths of theſe liquors are denominated, according to the ſyſtem propoſed by its inventor, having been long felt, and both one

Atkins on the ſpecific gravities of ſpirituous liquors.

and the other having accordingly in a great meaſure fallen into diſuſe amongſt all but thoſe immediately concerned in the collection of the duties, the lords of the treaſury during the laſt ſeſſion of parliament, applied for and obtained an act, empowering them to order any other hydrometer to be uſed for the purpoſes of the revenue, which might be found more conveniently applicable to them. This has of courſe rendered a review of the whole matter indiſpenſably neceſſary, and the authors of the pamphlet before us, whoſe attention to this ſubject is already ſufficiently known, have accordingly with great perſpicuity traced the principles on which alone an equitable ſyſtem can be eſtabliſhed.

After ſhewing the connection between the ſubject of the preſent tract, and the appreciation of our weights and meaſures in general, they proceed in the preface to ſtate the authorities on which they have founded their eſtimation of the weights of the known meaſures of diſtilled water. Theſe are principally the experiments of Sir George Shuckburgh Evelyn *, and thoſe of the French commiſſioners of weights and meaſures. From the former it appears to reſult, that the cubic inch of diſtilled water at 60° of Fahrenheit's thermometer, weighed in air at the ſame temperature when the barometer ſtands at 29½ inches, is equal to 252.506 troy grains, and from the latter, equal to 252.55, ſo that the authors take it to be under the circumſtances ſufficiently near 252½, and that this weight therefore correſponds with the ſpecific gravity of 1,000. Their appreciation of the cubic meaſure of the wine gallon is ſimilar to that of the board of exciſe, viz. that it contains 231. cubic inches, and they eſtimate the pound avoirdupoiſe at 7,000 grains troy. The preliminaries and a detail of the calculations and reaſons on which the deductions are founded, are very properly introduced in this place, as neceſſary to the underſtanding of their ſubſequent eſtimation of the ſpecific gravity of proof ſpirit.

The work itſelf treats of the ſubject under the following general heads; of the general relation between the ſpecific gravities and the ſtrengths and values of ſpirituous liquors, and the circumſtances by which the former are influenced; of the

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* See Phil. Journal, Quarto Series, Vol. iii. p. 97, &c. and Octavo Series, Vol. iv. p. 35. (No. for January laſt.)

Atkins on the ſpecific gravities of ſpirituous liquors.

ſtandard of proof; of over-proofs and under-proofs, and the modes of appreciating them; of Mr. Gilpin's experiments and tables; and of the means of adapting them to the preſent ſtandard, with various problems and rules for that purpoſe; and concludes with the two tables of the authors mentioned in the title-page.

The firſt chapter treats very perſpicuouſly of the ſubject in general, and the intricacy in which it is involved by the joint effects of concentration and change of temperature.

In the ſecond the authors proceed to the eſtimation of the ſtrength of proof ſpirit, as deducible from a looſe paſſage in an act of parliament paſſed in 1762, by which it is enacted, that " for the purpoſes of that act, each gallon of brandy or ſpirits of the ſtrength of one to ſix under hydrometer-proof, ſhall be taken and reckoned at 7 lbs. 13 oz. the gallon; and which ſeems to be the only clauſe in the ſtatute-book, in which the ſtrength of any kind of ſpirituous liquor is attempted to be defined by reference to its ſpecific gravity. The temperature of the liquor and ſome other circumſtances are here left to be aſſumed, which the authors have accordingly been under the neceſſity of doing, and from hence they deduce, that proof ſpirit may reaſonably be conſidered as having the ſpecific gravity of 920 at 60°. according to the common acceptation; and this they accordingly recommend as the future legal definition of that ſtandard.

The third chapter contains a diſtinct account of the various modes of compariſon hitherto in uſe, and of their reſpective inconveniences and defects. According to the ſyſtem propoſed by the authors, the denomination itſelf would at once indicate the real comparative value of the ſpirit in queſtion, by reference to the equivalent quantity of proof ſpirit, or that which would produce or be producible from 100 gallons of the former, by the addition of water to the ſtronger of the two, till they were reduced to the ſame degree of ſtrength; allowance being made in all caſes for the concentration which takes place by mixture, and the change in bulk and conſequent difference in value by meaſure, according to the exiſting temperature. Thus, an under-proof of which 100 gallons might be made up with water from 80 gallons of proof, would be called ſpirit of " 80" or " 20 under-proof," and a ſpirit of which 100 gallons would when reduced to proof, make 134, would be called " 134,

or

Atkins on the ſpecific gravities of ſpirituous liquors.

or "34 over-proof." We here find ſome arithmetical rules introduced for the purpoſe of illuſtrating the practical facilities reſulting from the uſe of ſuch a ſyſtem, and the chapter concludes with a ſection on the general conſtruction of the hydrometer, in which, however, the authors have modeſtly forborne to ſpeak particularly of thoſe of their own conſtruction, which are already ſo well known to the public.

In the fourth chapter, which treats of the experiments and tables of Mr. Gilpin, the author ſpeaks with its merited praiſe of this great work, and in the fifth they give a variety of rules, ſome of them very curious, for ſolving all the moſt uſeful problems reſpecting this ſubject from theſe tables, which they conſider as an authentic regiſter of experimental reſults. Theſe proceſſes are all founded on the ſuppoſition that proof ſpirit is of the ſpecific gravity of 920 at 60°.

The ſixth and laſt chapter contains the two tables of the authors already mentioned, with their uſe and application. Theſe tables in a great meaſure anſwer all the purpoſes of the more voluminous ones of Mr. Gilpin, from which they are calculated by the preceding formulæ, and that in a much ſimpler and eaſier manner, being adapted to the preſent ſtandard of proof, and giving the required per-centage and concentration by mere inſpection.

Having thus ſtated the outlines of this work, it would be to take notice of a circumſtance of minor conſequence if I remarked, that it is a very neat ſpecimen of typographical elegance. The ſubject is a moſt important one, and the philoſophical reader will be pleaſed to find it here treated in a manner which renders it no leſs worthy of the attention of the man of ſcience, than of thoſe whoſe commercial intereſts are involved in the event of the preſent inveſtigation.

A. B.

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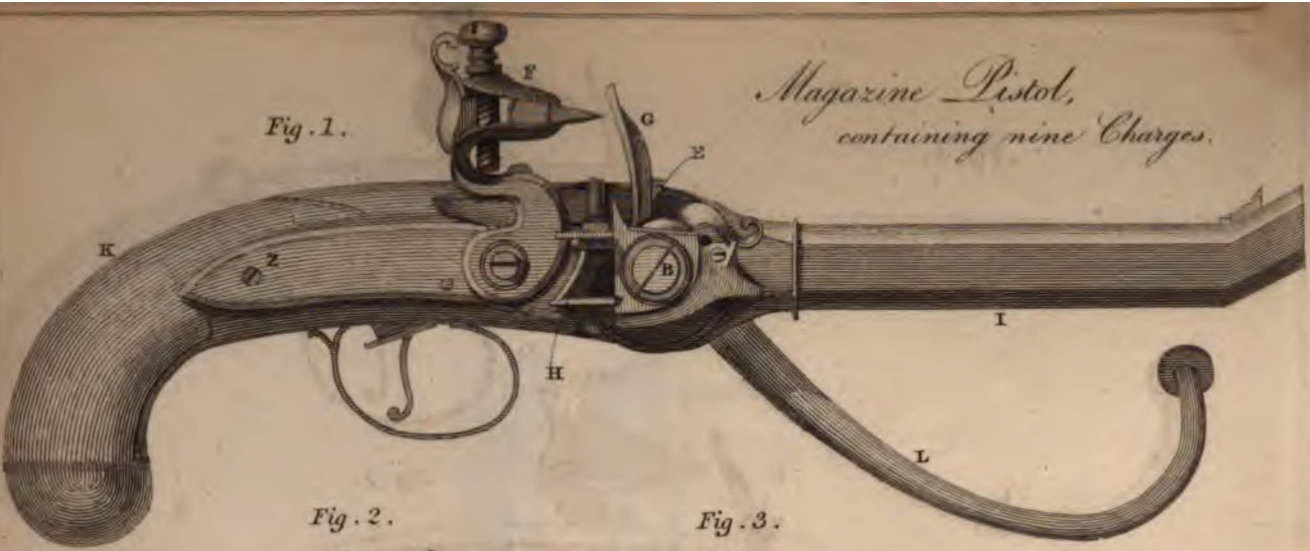

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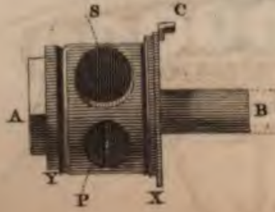

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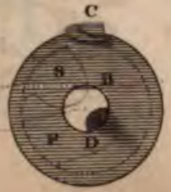

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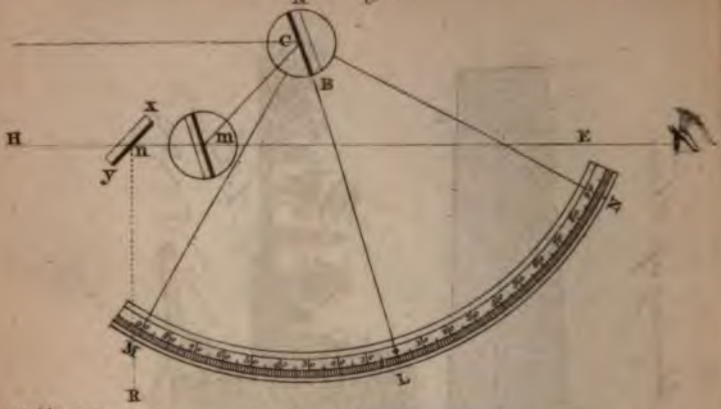

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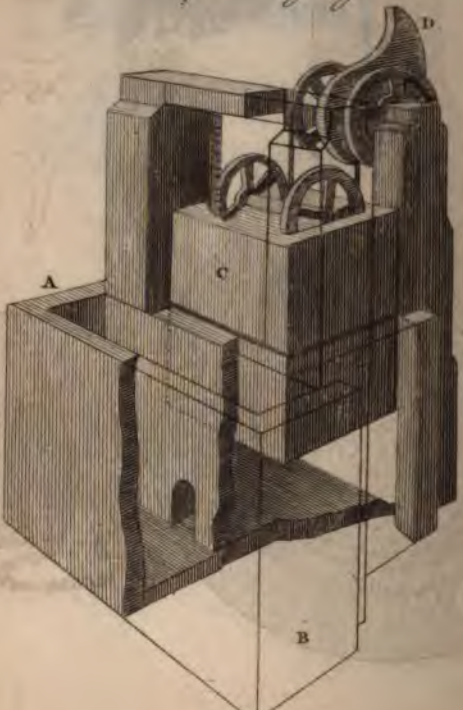

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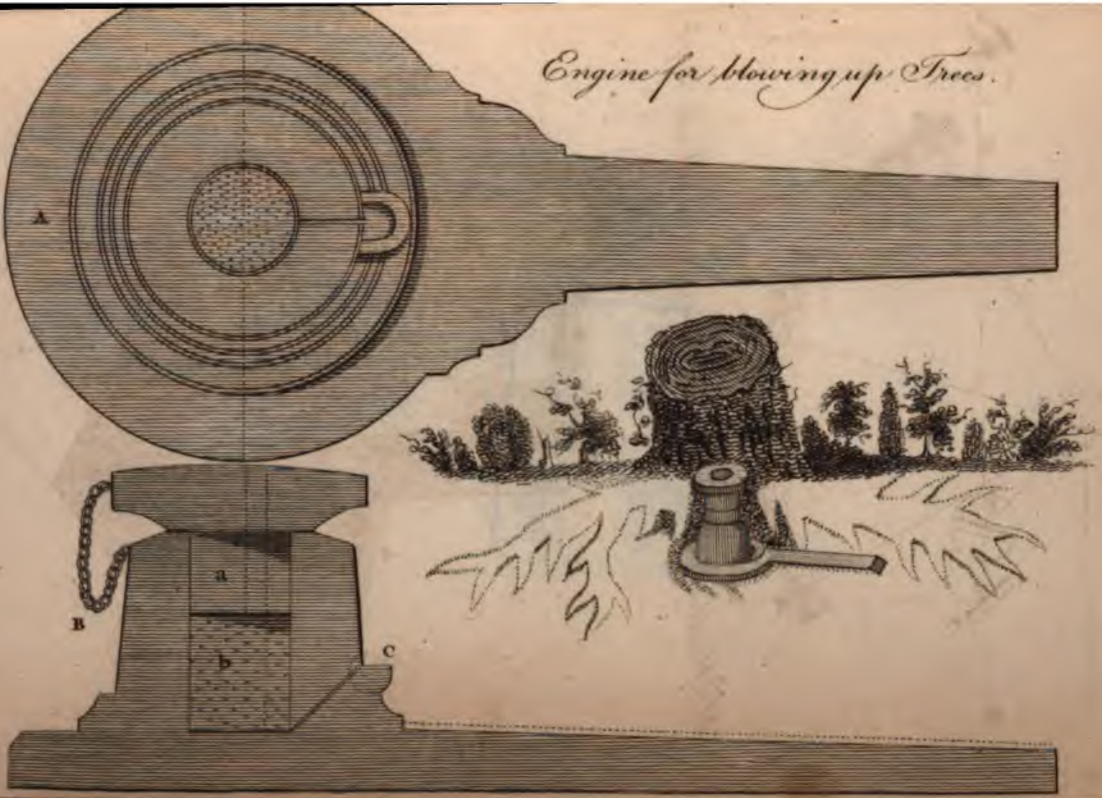

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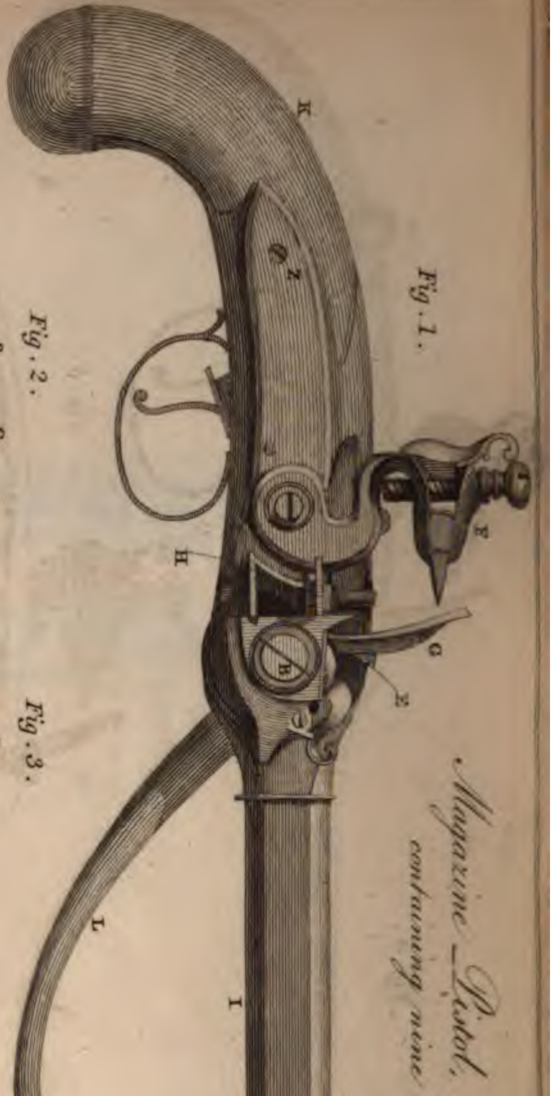

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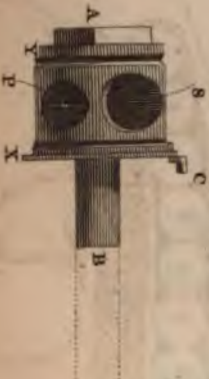

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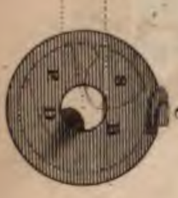

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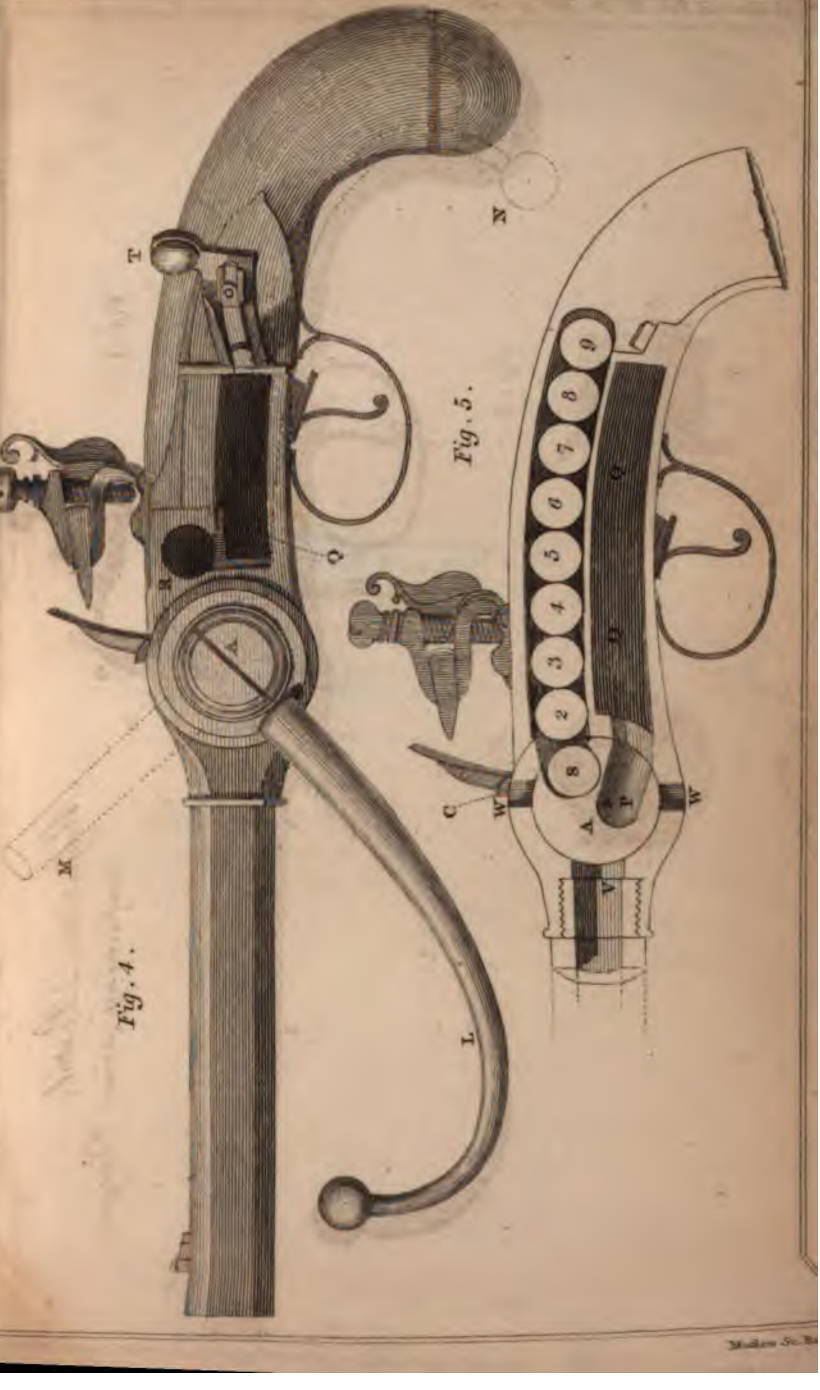
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